

# *The* SOUTHERN ECONOMIC JOURNAL

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A JOINT PUBLICATION OF THE SOUTHERN ECONOMIC ASSOCIATION  
AND THE UNIVERSITY OF NORTH CAROLINA

Volume XXII, Number 4

APRIL 1956

## *The* SOUTHERN ECONOMIC JOURNAL

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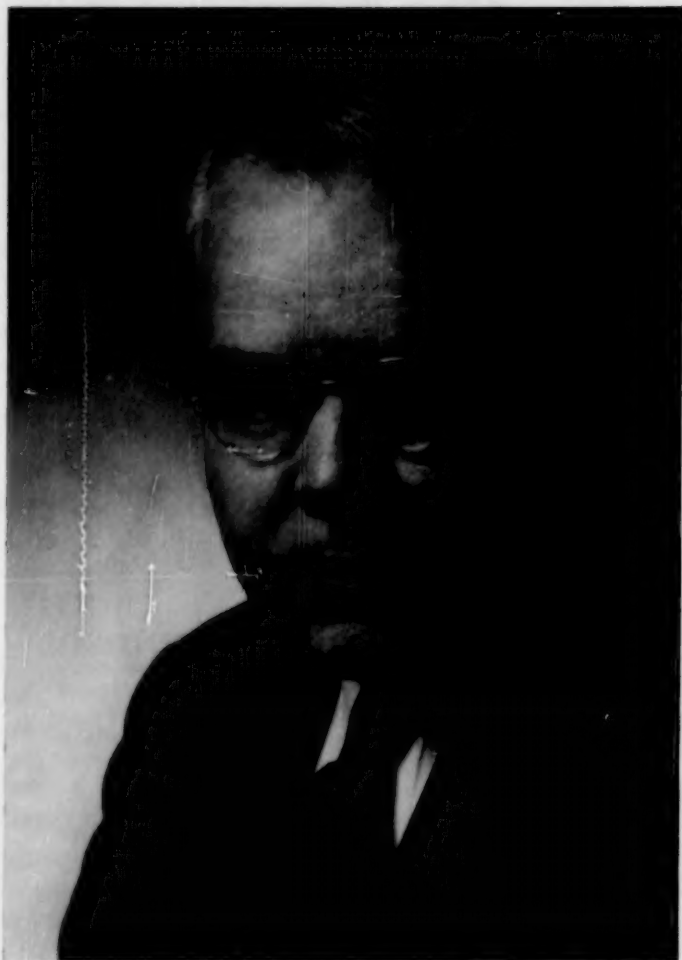
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# *The* SOUTHERN ECONOMIC JOURNAL

VOLUME XXII

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## CAPITAL IMPORTS AND THE BALANCE OF PAYMENTS<sup>1</sup>

JAMES C. INGRAM

*University of North Carolina*

The modern world is strangely ambivalent in its attitude toward international capital movements. On the one hand, the contribution of capital in the productive process is recognized, and it is regarded as an essential ingredient in the struggle to raise incomes throughout the world. On the other hand, capital (particularly foreign capital) is treated with suspicion and distrust, and numerous obstacles are placed in the way of its movement.

Fear of political domination and other non-economic factors unquestionably account for much distrust of foreign capital, but in some degree the attitude appears to rest upon economic considerations. Modern economics has stressed the demand side of investment almost to the exclusion of its supply side. The theory of capital movements has concentrated upon the short-run transfer problem with little or no attention to the effect on productive capacity. As a result, the idea has increasingly become accepted that an increase in domestic capital formation will threaten the foreign exchange reserves of a country because it produces a multiplier increase in money incomes, part of which will be spent for imports, with the result that imports may rise *more than* the initial inflow of foreign capital. Potential price effects reinforce this position. Entirely aside from interest or repayment of principal, an inflow of capital to finance investment activity may thus be regarded as a threat to international solvency of the receiving country.<sup>2</sup>

Where the operation of the new facilities is considered at all, it is sometimes regarded as an additional force tending toward balance of payments pressures on the ground that the increased incomes will again induce a rise in imports. Emphasis upon the possible stability of the marginal propensity to import has led economists to advise developing countries to channel investment into export industries in order to minimize the payments threat.

It is the principal argument of this article that these dangers have been exaggerated, and that explicit recognition of the supply side of new investment will help to eliminate the economic basis for the attitude now confronting foreign capital in much of the world.

<sup>1</sup> Part of this paper was presented at the Twenty-Fifth Annual Conference of the Southern Economic Association, Atlanta, Georgia, November 11, 1955. The author is indebted to Frank C. Child of Pomona College and Rashi Fein of the University of North Carolina for reading and criticizing this paper.

<sup>2</sup> P. T. Ellsworth, *The International Economy*, p. 818: "Were [foreign funds] provided in the forms of grants, . . . there would still be a balance of payments problem."

## I

The last decade has seen an extension of the analysis of income and employment to include the influence of the increase in capacity that accompanies net capital formation.<sup>3</sup> Several economists have shown that in addition to the increase in effective demand accompanying a rise in investment there is also an increase in real productive capacity when the newly produced capital goods go into operation. Efforts have been made to specify the rate of growth in effective demand that would be required to utilize the expanded capacity of the economy and thus achieve "full employment" in some sense.

Paralleling the development of this body of thought, but somewhat different from it in approach and form, we find efforts to analyze the problems of a country developing with the aid of foreign capital.<sup>4</sup> Here too a sharp distinction has been made between expenditures for new productive capacity and expenditures associated with the subsequent operation of that capacity.

We will not try to survey or summarize these contributions, but certain results and conclusions that seem to have been established will be used.

(a) Net investment in one time period—that is, new productive capacity constructed and installed, less depreciation—comprises a deflationary force when it is put into operation in a subsequent period. Such added capacity increases the full employment output of goods and services. For simplicity we assume that investment outlays made in one year will result in increased capacity in the next. That is, we assume a one-year gestation period.

(b) An increase in the annual rate of investment, consumption, or government expenditure will tend to produce a multiple increase in aggregate demand. In a closed economy, full employment can be maintained as long as the rate of growth in aggregate demand matches the growth in capacity caused by the operation, this year, of last year's net investment.

(c) A third result, less explicitly stated than the others, is that the usual multiplier theory does not apply to increases in income associated with the

<sup>3</sup> E. D. Domar, "Capital Expansion, Rate of Growth, and Employment," *Econometrica*, April 1946, pp. 137-47; "Expansion and Employment," *American Economic Review*, March 1947, pp. 34-55; "The Problem of Capital Accumulation," *ibid.*, Dec. 1948, pp. 777-94; R. Eisner, "Underemployment Equilibrium Rates of Growth," *ibid.*, March 1952, pp. 43-58; W. Fellner, "The Capital-Output Ratio in Dynamic Economics," in *Money, Trade and Economic Growth* (New York, 1951); D. Hamberg, "Full Capacity vs. Full Employment Growth," *Quarterly Journal of Economics*, Aug. 1952, pp. 444-49; R. F. Harrod, *Towards a Dynamic Economics* (London, 1948); J. R. Hicks, *Contribution to the Theory of the Trade Cycle* (Oxford, 1950); H. Pilvin, "Full Capacity vs. Full Employment Growth," *Quarterly Journal of Economics*, Dec. 1953 (with Comments by Harrod and Domar); S. C. Tsiang, "Accelerator, Theory of the Firm and the Business Cycle," *ibid.*, August 1951, pp. 325-41.

<sup>4</sup> J. J. Polak, "Balance of Payments Problems of Countries Reconstructing with the Help of Foreign Loans," *Quarterly Journal of Economics*, Feb. 1943. Reprinted in H. S. Ellis and L. A. Metzler, eds., *Readings in the Theory of International Trade* (Blakiston, 1949); N. S. Buchanan, *International Investment and Domestic Welfare* (New York, 1946), Ch. 6; A. E. Kahn, "Investment Criteria in Development Programs," *Quarterly Journal of Economics*, Feb. 1951, pp. 38-61; H. B. Chenery, "The Application of Investment Criteria," *ibid.*, Feb. 1953, pp. 76-96; F. Pazos, "Economic Development and Financial Stability," *I. M. F. Staff Papers*, Oct. 1953, pp. 228-53; and many others.

operation of new productive capacity in exactly the same way that it applies to changes in income caused by autonomous changes in investment, consumption, or government expenditures. The reason is that incomes earned in the operation of a new facility are exactly matched by the value of the new output of currently saleable goods. Even if the whole of the new income is spent, the rise in demand is accompanied by an equal increase in supply.<sup>5</sup> If all of the new output is sold on the domestic market, and if the new income-receivers spend all of their new incomes there, no further effects upon income and output will be produced. No multiplier is involved because the increase in demand does not precede the increase in supply, as before. Instead, the two are coordinate.<sup>6</sup>

(d) A corollary to (c) above is that the usual analysis of the foreign trade multiplier does not apply to increases in income associated with the operation of new productive capacity in exactly the same way that it applies to autonomous changes in investment, consumption or government expenditures. When an autonomous increase in (say) investment occurs, national income tends to increase by a multiple of the change in investment, and it is commonly assumed that imports<sup>7</sup> will rise in some constant ratio to income. Since nothing has happened to stimulate exports,<sup>8</sup> it is concluded that the increase in investment adversely affects the balance of trade.

But where we consider the rise in income from the operation of a new productive facility, a different conclusion emerges. Such an increase in income does not necessarily lead to any increase in imports, and if imports do rise the chances are that exports will rise in equal or greater amount, thus maintaining or improving the balance of trade. For this to follow, we need to specify only two conditions: (1) the new output is successfully sold on the market (either domestic or foreign) in addition to goods previously sold there,<sup>9</sup> and (2) the monetary authorities permit an increase in MV only in proportion to the increase in real output, thus leaving the price level unchanged.<sup>10</sup>

<sup>5</sup> Here we assume the new output to be a net increase. If factors must be drawn from other employments the same reasoning will apply to the new output minus the required curtailment in other output.

<sup>6</sup> The timing of income disbursements and the marketing of the new output might produce some leads and lags, but we ignore these in this analysis.

<sup>7</sup> We shall use "imports" and "exports" to refer to the entire amount of current account debits and credits, respectively.

<sup>8</sup> Throughout this paper we shall ignore the repercussions in the rest of the world. This is done to simplify the argument, but it may be partially justified by an assumption that the country under examination is "very small" in comparison to the rest of the world, so that its actions will have scarcely noticeable effects. For a thorough treatment of reciprocal repercussions, see F. Machlup, *International Trade and the National Income Multiplier* (Blakiston, 1943).

<sup>9</sup> This proviso merely means that the new venture succeeds. It is evident that if money is spent for unproductive purposes, or if entrepreneurs made mistakes of judgment, no "effective supply" will reach the market to offset the augmented incomes.

<sup>10</sup> The reason for this condition is that we wish to eliminate the complicating factor of price changes, and thus to isolate the effect of the change in income. As Kahn has forcefully pointed out, the reason Polak found a net rise in imports in the operation of his Class III industries is simply that he assumed inflationary financing of the purchase of their output. Cf. Kahn, *op. cit.*, pp. 42-48. This will be further discussed in Part III.

The reason why the balance of trade will remain the same or improve, given these conditions, is simply that as the new goods (call them "Z") are sold in the market, expenditures are shifted from other products onto the Z-goods, and these "other products" become available for sale to the Z-producers. The shift of expenditures may be made by the consumers, government, or entrepreneurs of the country concerned, or it may be made by the "rest of the world." No matter whose expenditures shift onto Z-goods, the shift releases other goods and services for sale to Z-producers as long as the purchaser does not change his propensity to spend out of his given income. If no one shifts, then our condition (1) requires the Z-producers themselves to have bought the output of the Z-industry.<sup>11</sup>

This last result is less commonly made explicit in the literature, but Kahn develops the argument in detail.<sup>12</sup> It leads to the conclusion that the marginal propensity to import may be zero, and that, given conditions (1) and (2) above, the balance of trade will be improved or at worst unchanged by an increase in income arising from the utilization of new capacity. Such results have been observed for particular countries.<sup>13</sup>

Many complex adjustments are taken for granted in the above analysis, or are ruled out by our conditions. For example, when Z-producers buy domestic goods with their new incomes, it is unlikely that they will wish to buy exactly the same goods that are released as other spenders shift to Z-goods. However, if they do not, then shifts of resources and prices will take place. Similarly, when domestic goods are released for export, we cannot be sure that foreigners will wish to buy precisely those goods, or that the elasticity of foreign demand will be infinite. All such complications, important as they are, have been ignored in

<sup>11</sup> In the foregoing we assume that no change occurs in the basic functions of the system. I.e., no shift occurs in the propensity to consume, no change in profit expectations, no change in attitudes toward cash balances, no change in tax rates or total government expenditures. In other words we rule out any autonomous changes which might obscure the effect of the single change we are examining.

<sup>12</sup> Kahn, *op. cit.*, pp. 43-48. As a matter of fact, the usual treatment of the mechanism of adjustment in the balance of payments completely ignores the capacity effect being stressed here. For a recent example, see the excellent monograph by G. Haberler, *A Survey of International Trade Theory*, International Finance Section, Princeton University, 1955, pp. 34-48.

<sup>13</sup> E.g., the following account of Indian experience fits neatly into this hypothesis:

"In an underdeveloped country like India, where goods are to a considerable extent produced for direct consumption by their producers and the economic activities of the community are confined largely to the satisfaction of minimum wants, the conventional approach to imports as a function of income can be accepted only with reservations. Under certain conditions, the imports of an underdeveloped country, far from rising with an increase in income, may actually show a decline. In contrast to the situation in a developed economy, . . . a large part of any increase in farm production in India . . . is absorbed by the farmers . . . and only part of their increased product is exchanged for money. To the extent that the surplus is marketed, the rest of the domestic community might reduce its demand for imports. Furthermore, the money incomes the agriculturists obtain . . . are likely to give rise to additional demands for basic non-farm consumption goods, which will be met mainly by increased production of domestic industries and to a much lesser extent by imports." S. Kumarasundram, "India's Postwar Demand for Imports," *I. M. F. Staff Papers*, Feb. 1955, p. 311.

our analysis because we wish to isolate the balance-of-payments effect of the successful operation of a new industry.

It is advisable at this point to specify certain other assumptions that have not been spelled out in the above discussion. The capital-importing nation is assumed to have underemployed or unemployed labor or land which can be drawn into productive uses with the aid of capital imports. All such unutilized resources need not be present at the beginning of the time sequence, however. Growth of population and the settlement of new lands may account for much of the increase in the supply of cooperating productive factors. These, together with a progressive shift in techniques, could provide enough flexibility of supply to permit output to expand over time without much change in prices. Where an abundance of labor causes its marginal productivity to be near zero or even negative, additional basis for expansion of supply is present. We also assume that exchange rates are held constant by the operation of a stabilization fund in the central bank, and that exchange transactions are free.

The conclusion reached above, that no adverse effect on the balance of payments will arise from increases in income associated with the operation of new industries, is no green light to indiscriminate expansion of economic activity in developing countries. That the output must be sold on the market at a price which will cover costs sharply limits the scope of action. It is, indeed, an obvious and homely criterion for successful capital formation. We offer no formula for selecting such potentially successful industries; we wish merely to show that this is the hard problem of the developing country. When they find such industries, they need not fear balance-of-payments pressures from their operation, but only from the initial investment in them.

## II

When we separately analyze the two processes of capital formation and current operation of an industry, the balance-of-payments "problem" thus appears less intractable. This follows from the familiar proposition, alluded to above, that an increase in supply is *per se* a deflationary force. When a rise in income occurs as a result of an expansion of the supply of immediately saleable goods, it is likely that a portion of the new incomes will not reach the market to swell the demand for goods and services. First, individuals and firms may save a portion of their new incomes. Unless a corresponding increase in expenditure is made by other individuals and firms, such personal or business savings would have a deflationary influence since we assume that the entire output of newly produced goods will enter the market. (Any such corresponding increase in expenditures would represent a shift in consumption-saving patterns and thus would violate our assumptions.) Second, with fixed tax rates part of the new income would accrue to the government. By our assumptions, no change in tax rates or in expenditures is permitted. Therefore, if the leakage through private and governmental saving is a positive fraction of the new income, the net result will be deflationary in tendency.

It follows that the expansion of output through the operation of a new indus-



try will, in the "normal" case, strengthen the balance of payments because the goods unsold in the domestic market are available for export (or because domestic buyers switch to domestic goods and reduce imports). No multiplier-effect will arise to eliminate the surplus in the trade balance because it is precisely offset by the rise in voluntary savings. A developing country may, therefore, proceed to expand domestic investment without any foreign capital so long as it balances the demand effect of the increments to new investment against the net deflationary effect of the operation of the productive facility so constructed. All it must ensure is that the goods produced are saleable, and that inflationary financing is avoided. (This subject is further pursued in Part III.)

Now let us examine the implications of this analysis for the type of product that a developing economy should choose in order to minimize foreign exchange pressures and maximize income growth. One conclusion is that the distinction between export (and import-competing) goods and home-market goods is not particularly useful. In the first place, no sharp distinction can be drawn between these two types of goods because any good successfully sold on the home market may attract buyers away from imports. Therefore, even though the good cannot possibly be exported—e.g., apartment house rentals or night-club entertainment—the successful sale of it in the home market may reduce imports and thus strengthen the payments position.<sup>14</sup> Deliberate concentration on goods for the export market is likely to increase the initial surplus in the balance of trade, but to the extent that the surplus is not matched by a rise in saving, a multiplier-effect will follow which will lead to an induced rise in imports as well as to increased domestic inflationary pressure. To the extent that the export surplus is matched by a rise in saving, it will appear even if the newly constructed industry produces strictly home-market goods. Kahn showed that the real reason some writers (e.g. Polak and Buchanan) found a significant difference between the payments effect of the operation of export and home-market industries is that they assumed the output of the latter to be financed in an inflationary manner.<sup>15</sup> If money incomes rise by the same amount as the value of the extra output, no such net rise in imports will occur. It follows that the oft-heard advice to poor countries, to avoid expanding the production of goods for the home market, is incorrect. They should instead encourage the production of any goods they can successfully sell in any market, domestic or foreign, without inflationary financing.

A second conclusion regarding the type of product is that, *ceteris paribus*, a country with scarce foreign-exchange reserves should concentrate on so called "capital-light industries," or those in which the ratio of output to capital is relatively large. In his analysis of this criterion for the allocation of new invest-

<sup>14</sup> Such services might of course be sold to tourists and thus comprise "export goods" more directly.

<sup>15</sup> Polak, *op. cit.*, Buchanan, *op. cit.* Kahn, *op. cit.*, pp. 45-46, has stressed that Polak defined his Class III goods as "goods sold on the home market in addition to those previously sold, and in excess of the increase in demand owing to the rise of incomes." The last clause in this definition violates our provision that the authorities permit a rise in MV only to match the increase in real output.

ment, Kahn concluded that it was not valid, but he seems to have overlooked an important aspect of his own argument.<sup>16</sup> The problem of the developing country is to achieve the highest possible rates of investment and income growth with a given inflow of foreign capital, or with a given stock of foreign-exchange reserves. Increments to investment produce a multiplier-effect upon money incomes, an increase in imports, and thus create a drain on exchange reserves. But when the new capital facilities begin operation, the increase in incomes is matched by an increase in supply, and as we have seen it is likely that some portion of the new incomes will be saved, thus freeing part of the current production for export (or to replace imports). The larger the value of the current output of the new industry, the larger will be the increase in money incomes, and therefore the larger will be the money value of the leakages from the income stream. Consequently, if a country concentrates its new investment in industries in which the output-capital ratio is high, the deflationary force of the new output will be greater and therefore more adequate to counteract the inflationary effect of the new investment itself. It is possible that a country could, by a judicious choice of industries in which to invest, manage to carry on quite a respectable program of capital formation without any net drain on foreign exchange reserves. To do so it must heed our two basic conditions: the new enterprise must be capable of selling its output on the market at a price which will cover costs, and such sales must not be financed in an inflationary manner (i.e., by a creation of money demand in excess of the rise in incomes accompanying the new output).

It is therefore in the deflationary influence of an increase in supply that we find the economic basis for emphasis upon capital-light industries. The development of such industries will encourage the growth of domestic savings and thus permit more rapid formation of capital.

It does not follow that capital intensity is the sole or necessarily even the dominant criterion to be applied in allocating investment in developing countries. Kahn properly stresses the fundamental role of (social) marginal productivity. But unless this concept is defined so broadly as to be meaningless as a guide to policy,<sup>17</sup> it is likely that it might conflict with capital intensity in circumstances in which the choice of the latter would be preferable. Suppose, for example, that social marginal productivity were equal for two investments, *A* and *B*, but that *A* has an output-capital ratio of 1:2, while *B* has a ratio of 2:1. The choice of *B* seems indicated on the grounds discussed above. Then suppose the social marginal productivity of *A* were slightly higher than that of *B*. The choice of *B* might still be indicated. Just where the advantage of a greater social marginal productivity outweighs the advantage of a higher output-capital ratio is not easy to determine. Nevertheless, to the extent that the objective is rapid growth in income and capital formation with a minimum of payments pressure,

<sup>16</sup> Though he ruled it out on purely theoretical grounds, Kahn did admit this rule as a useful rule-of-thumb in practical situations. *Op. cit.*, pp. 50-53.

<sup>17</sup> If all direct and indirect consequences of a new investment, including even its effect upon the balance of payments, are considered in the calculation of social marginal productivity, the usefulness of the concept as a criterion of action is virtually nil. Such a broad definition is used by Chenery, *op. cit.*



low capital intensity is a valid criterion of choice. This may sometimes conflict with social marginal productivity, but it will often coincide with it.

### III

When we allow for the deflationary influence of expanded current output, it appears that a nation can theoretically achieve a steady increase in its rate of capital formation without experiencing an adverse payments balance. Furthermore, with a given annual inflow of foreign capital there is a maximum rate of increase in capital formation that can be maintained. We turn to a discussion of the determinants of these rates. The permissible rates will clearly depend on the special circumstances of each nation; here we will try merely to specify some general characteristics which will influence the actual outcome.

In this discussion we will ignore the repayment problem. We assume that equity capital is involved, and that if the original investor wishes to liquidate his investment he simply sells his claim to another foreign buyer.<sup>18</sup> The balance of payments of the capital-receiving country is not affected by such shifts of ownership.

For simplicity we also ignore the foreign-exchange problems involved in remitting profits. The effect of our treatment is to overstate the increment to domestic saving, since part of the profits earned will actually be remitted abroad. Alternatively, we may assume that profits are re-invested, and that the inflow of foreign capital is higher by this amount than is indicated in the calculations below.

First let us consider the maximum rate of increase in investment that a country could sustain with *no* inflow of foreign capital. The increase in investment in the current year brings about a multiplier rise in money incomes, the size of the multiplier depending on the size of home and foreign leakages. Ignoring foreign repercussions,

$$\Delta Y_I = \Delta I \left( \frac{1}{s + m} \right),$$

where  $s$  = marginal propensity to save,  $m$  = marginal propensity to import, and  $\Delta Y_I$  = the eventual rise in income associated with a given change in  $I$ . As income rises by  $\Delta Y_I$ , imports will rise by  $m \cdot \Delta Y_I$  and, since there is no reason to expect any change in exports,<sup>19</sup> the balance of trade will be weakened by this

<sup>18</sup> This is the principal form of liquidity available to domestic investors, and it is the only form that can reasonably be expected by the holder of foreign assets. It would represent a considerable advance if Professor Knight's thesis that capital must be regarded as a permanent, perpetual fund were to be accepted in discussions of international investment. This is not to deny that debtor nations can pay their debts. Many have done so. The point is simply that such shifts in creditor-debtor positions are longrun phenomena that are very inadequately understood. In plans for the short run, say 8-10 years, the net repayment of capital imports need not always be contemplated.

<sup>19</sup> We are assuming that no price changes occur. Even without price changes, exports might decline as domestic buyers receive additional incomes. However, our assumption of perfectly elastic supply functions covers this possibility.

amount. Even though we assume that resources are available to permit an expansion of output to match the rise in demand for domestic goods, the deterioration in the balance of trade will occur.

Now during the current year (let us assume) the entire net investment of the preceding year ( $I_{t-1}$ ) goes into operation. Income and output simultaneously rise on this account—i.e., in *addition* to the rise engendered by the current increase in investment.<sup>20</sup> The extent of the increase in income will depend on the ratio of output to capital in the operation of the new facilities. That is,

$$\Delta Y_o = I_{t-1} \cdot \sigma,$$

where  $\sigma$  = the ratio of output to capital for the investment of the preceding year, and  $\Delta Y_o$  = the change in  $Y$  associated with operation of the new facilities.

Assuming that the receivers of this new income have the same marginal propensity to import as the rest of the population, imports will rise as  $Y$  rises.<sup>21</sup> But, since all the new output is presumed sold, either exports must rise by the same amount as do imports, or other imports must fall. For example:

- (1) Exports will rise by the same amount as imports if other income receivers switch \$100 from other domestic goods to buy the new  $Z$ , for if  $Z$ -producers buy (say) \$80 of domestic goods, then the remaining \$20 worth of domestic goods are "freed" for export.
- (2) Other imports will fall if other income receivers switch from imports and other domestic goods in their usual proportions to buy the new  $Z$ , and their decline in imports will be equal to the rise in imports of  $Z$ -producers. (This involves a decline in the average propensity to import. See note 21.)

Assuming further that the receivers of the new income have the same marginal propensity to save as the rest of the population (including here business saving and government surpluses), a further rise in exports will occur on this account, thus producing a net improvement in the balance of trade. This follows from our assumption that the whole of the new output is successfully sold on the market. To the extent that domestic savings increase, the output is not sold at home, and is therefore "released" for export.

Therefore, the foreign leakage ( $m$ ) leaves the balance of trade unchanged, and the home leakage ( $s$ ) produces a net improvement in it. The size of the export

<sup>20</sup> At the risk of repetition, it should be made clear that we assume that  $\Delta Y_i$  represents an increase in output (to the extent that expenditures for domestic goods and services increase) as well as in money income. This is different from Domar's treatment, for he lets  $\Delta Y_o$  represent an increase in real output and  $\Delta Y_i$  represent the increase in money demand required to buy the new output. (Domar of course uses other symbols than these. Cf. his "Expansion and Employment," *op. cit.*) Here we assume that an increase in investment produces an increase in expenditure which brings forth an increase in output in the economy. Then when new facilities go into operation, there is an *additional* increase in both output and money income. See Part I, where assumptions about resource availability are set forth.

<sup>21</sup> As observed in Part I above, the marginal propensity to import may be zero in such cases. We assume it remains constant in the country under examination, but, since exports must therefore increase, this amounts to assuming a *rise* in the (average) propensity to import in the rest of the world!

surplus resulting from the operation of the new capacity will be equal to  $\Delta Y_o \cdot s$ , since

$$\begin{aligned}\Delta M_o &= m \cdot \Delta Y_o \\ \Delta X_o &= (s + m) \Delta Y_o \\ \Delta X_o - \Delta M_o &= (s + m) \Delta Y_o - m \cdot \Delta Y_o = s \cdot \Delta Y_o\end{aligned}$$

Clearly, the size of the output/capital ratio is an important determinant of the absolute size of the increase in savings. The larger " $\sigma$ " is, the greater will be the increase in savings and the consequent improvement in the trade balance.

The problem of the developing country is to equate the increase in imports resulting from the multiplier-effect of the current increase in  $I$  with the increase in net exports ( $X - M$ ) resulting from the expansion of current output as last year's addition to capacity goes into operation. In this way it achieves two types of increased income. The greater the rise in income from operation of new capacity, the greater the increase in net exports (on that account), and therefore the greater the increase in investment that can be supported in the current year.

The increase in imports from new investment is:

$$\Delta M_I = m \cdot \Delta Y_I = m \left( \Delta I \cdot \frac{1}{s + m} \right)$$

The operation of the new facilities added last year produce the following effect upon the trade balance:

$$\Delta X_o - \Delta M_o = s \cdot \Delta Y_o = s \cdot I_{t-1} \cdot \sigma.$$

Our problem is to equate the deficit arising from a current increase in  $I$  with the surplus arising from the operation of  $I_{t-1}$ , or:

$$\Delta X_o - \Delta M_o = \Delta M_I.$$

It follows that:

$$m \left( \frac{\Delta I}{s + m} \right) = s \cdot I_{t-1} \cdot \sigma,$$

and

$$\frac{\Delta I}{I_{t-1}} = s \left( \frac{s + m}{m} \right) \cdot \sigma.$$

That is, the permissible rate of increase in domestic investment is a function of the output-capital ratio and the propensities to save and import. The greater the rise in income from the operation of new facilities (i.e., the higher " $\sigma$ " is), the greater the increase in  $I$  that can be sustained without payments pressures. This is the economic case for capital-light industries. We also observe that the higher " $s$ " is, the greater the permissible rate of increase in  $I$ , and the higher " $m$ " is, the smaller the permissible rate.

This analysis may be illustrated by an arithmetical example. Suppose a country has the following characteristics:

$$\begin{aligned}s &= .1 \\ m &= .2 \\ \sigma &= 1.0\end{aligned}$$

Then  $\Delta I/I_{t-1} = \frac{s(s+m)\sigma}{m} = 15$  per cent. If  $I_{t-1} = 1,000$ , then  $\Delta I$  can be 150, or this year's  $I$  can rise to 1,150. The increase in  $I$  has the following effects upon income:

Income Period	$\Delta Y$	$\Delta C$	$\Delta S$	$\Delta M$	$\Delta X$	$\Delta I$	$\Delta Y$
1						150	150
2	150	105	15	30			105
3	105	73.5	10.5	21			73.5
.	.	.	.	.		.	.
.	.	.	.	.		.	.
.	.	.	.	.		.	.
$\infty$							
	500	350	50	100	—	150	500

The trade balance turns adversely by 100. While this multiplier is working itself out, last year's net investment is put into operation, producing new income in the amount of  $I_{t-1} \cdot \sigma$ , or 1,000. This new income is disposed of as follows:

$$\begin{aligned}\Delta C &= 700 \\ \Delta S &= 100 \\ \Delta M &= 200\end{aligned}$$

But if  $\Delta C_0$  rises by only 700, then the remaining 300 of new output must have been exported. Thus the total change in exports (300) equals  $\Delta M_I$  (100) plus  $\Delta M_O(200)$ .<sup>22</sup> In the next year,  $\Delta I$  can rise to 15 per cent of 1,150, or 172.5. That is,  $I_{t+1}$  can rise to 1,322.5 without an adverse trade balance appearing.

The influence of " $s$ ," " $m$ ," and " $\sigma$ " is depicted in Table I, where it may be seen that the rate of increase in investment varies directly with " $s$ " and " $\sigma$ ," and inversely with " $m$ ." The really significant point is, however, that these growth rates do not create balance of payments pressures. Furthermore, the

<sup>22</sup> We could have assumed the new goods were sold on the home market, in which case other imports would have declined, still leaving  $\Delta X - \Delta M = 0$ . This is a reasonable sequence in the developing country—for newly constructed plants to supply goods to satisfy the growing demand as investment steadily expands. To some extent the country can create its own market for new output as it goes along.

TABLE I

Permissible Rates of Growth in Annual Investment Under Various Conditions Without Adverse Payments Effects

<i>s</i>	<i>m</i>	$\Delta I$ as a percentage of $I_{t-1}$			
		$\sigma = .2$	$\sigma = .5$	$\sigma = 1.0$	$\sigma = 2.0$
.1	.1	4%	10%	20%	40%
	.2	3	7.5	15	30
	.3	2.7	6.7	13	27
.2	.1	12%	30%	60%	120%
	.2	8	20	40	80
	.3	6.7	16.7	33	67
.3	.1	24%	60%	120%	240%
	.2	15	37.5	75	150
	.3	12	30	60	120

$$\frac{\Delta I}{I_{t-1}} = \frac{(s \cdot \sigma)(s + m)}{m}$$

rates may be compounded, and a country with  $s = .1$ ,  $m = .2$ , and  $\sigma = 1.0$ , could double its annual rate of investment in about five years.

With a given annual inflow of foreign capital, the permissible rise in  $I$  will be greater. Investment may now reach a level at which the import drain,  $\Delta M_I$ , exceeds the rise in net exports,  $\Delta X_o - \Delta M_o$ , resulting from the operation of last year's investment, by the amount of the capital inflow. That is

$$(\Delta X_o - \Delta M_o) + V = \Delta M_I, \text{ where } V = \text{capital inflow.}$$

Hence,

$$s \cdot I_{t-1} \cdot \sigma + V = m \left( \frac{\Delta I}{s + m} \right)$$

If we let  $V$  equal some fraction " $k$ " of  $I_{t-1}$ , then

$$V = k \cdot I_{t-1},$$

and

$$s \cdot I_{t-1} \cdot \sigma + k \cdot I_{t-1} = m \left( \frac{\Delta I}{s + m} \right)$$

$$\frac{\Delta I}{I_{t-1}} = \frac{(s \cdot \sigma + k)(s + m)}{m}$$

Our earlier formulation can also be put in this form, with  $k = 0$ :

$$\frac{\Delta I}{I_{t-1}} = \frac{(s \cdot \sigma + 0)(s + m)}{m},$$

from which it can be observed that the effect of a positive " $k$ " is to increase the permissible rate of growth in  $I$  by the fraction  $k/s \cdot \sigma$ . For example, in the illustra-

tion worked out above for  $s = .1$ ,  $m = .2$ , and  $\sigma = 1.0$ , if  $k = .1$  the permissible rate of growth in  $I$  will double. Thus

$$\frac{\Delta I}{I_{t-1}} = \frac{(.1 + .1)(.1 + .2)}{.2} = 30 \text{ per cent,}$$

which is twice as great as we obtained with  $k = 0$ .

Foreign capital is particularly helpful, therefore, where either " $s$ " or " $\sigma$ " (or both) is small. In Table II we show the new values for  $\Delta I/I_{t-1}$  on the assumption that " $k$ " = .1. For small " $s$ " and " $\sigma$ ," the effect is to increase greatly

TABLE II  
Permissible Rates of Growth in Annual Investment Where Capital Imports Equal Ten Per Cent of Last Year's Net Investment. No Payments Pressure

$s$	$m$	$\Delta I$ as a percentage of $I_{t-1}$ where $k = .1$			
		$\sigma = .2$	$\sigma = .5$	$\sigma = 1.0$	$\sigma = 2.0$
.1	.1	24%	30%	40%	60%
	.2	18	22.5	30	45
	.3	16	20	26.7	40
.2	.1	42	60	90	150
	.2	28	40	60	100
	.3	23	33	50	83
.3	.1	64	100	160	280
	.2	40	62.5	100	175
	.3	32	50	80	140

$$\frac{\Delta I}{I_{t-1}} = \frac{(s \cdot \sigma + k)(s + m)}{m}$$

TABLE III  
Percentage Increase in the Rate of Growth ( $\Delta I/I_{t-1}$ ) When " $k$ " changes from Zero to Ten Per Cent

$s$	$m$	$\frac{\text{Rate of Growth with } k = .1}{\text{Rate of Growth with } k = 0}$			
		$\sigma = .2$	$\sigma = .5$	$\sigma = 1.0$	$\sigma = 2.0$
.1	.1	600%	300%	200%	150%
	.2	600	300	200	150
	.3	600	300	200	150
.2	.1	350	200	150	125
	.2	350	200	150	125
	.3	350	200	150	125
.3	.1	267	167	133	117
	.2	267	167	133	117
	.3	267	167	133	117



the permissible rate of growth. E.g., for  $s = .1$  and  $\sigma = .2$ , the rate of growth is augmented by five times the previous rate, or from 4 per cent to 24 per cent. When " $\sigma$ " is large, on the other hand, the need for foreign capital is greatly reduced. Or, in other words, the extent to which capital imports enable a nation to expand its rate of growth above the rate it can sustain unaided will diminish as the output/capital ratio increases. This relationship is depicted in Table III, where it can be seen that for our illustrative values of " $s$ ," " $m$ ," " $\sigma$ ," and " $k$ ," the proportionate increase in  $\Delta I/I_{t-1}$  varies inversely with " $s$ " and " $\sigma$ ," but is unaffected by " $m$ ."

## IV

Explicit recognition of the supply effects of new investment suggests that the outlook for developing economies may be more promising than it is often considered to be, at least in regard to the balance of payments problem. Successful growth requires, above all, that entrepreneurs perceive and undertake investment projects which are economically feasible in the sense that the output produced may be sold for a price covering the cost of production. The problem of finding these projects remains; all we have done is to show that investment in and operation of such facilities is less likely to produce payments difficulties than is generally recognized.

The analysis also suggests that the allocation of new investment between export and domestic goods is not of much importance so long as the monetary authority does not inflate the money income faster than output increases. In the operation of new industries, inflation is the villain producing payments pressures, as Kahn has said. If inflation is avoided, our conclusion is that new production resulting from the operation of new facilities is, on balance, deflationary, and that the higher the output/capital ratio, the greater the rate of growth that can be sustained.

Our analysis provides no panaceas, however, for we have imposed some highly restrictive conditions. Specifically, we have assumed that unemployed or underemployed resources exist, or that some factor supply is growing through time. Where such previously unused resources exist, the size of the output/capital ratio will be higher than it will be where the increase in output is limited to the contribution of the increment of capital. (I.e., we assume that the new capital serves as an enabling factor which permits the utilization of hitherto unused resources. Thus the earnings of these other resources will be part of the increment to output in the ratio " $\sigma$ ." ) Furthermore, we have assumed a stable price level, which means that we assume the real elasticity of supply to be infinite. To the extent that increasing expenditure for the factors produces a rise in their prices, induced imports may be quite different than described above. Yet we must try to isolate the various forces, and ours is an extremely partial picture.<sup>23</sup>

Implications of the above hypothesis need to be subjected to empirical test,

<sup>23</sup> Machlup, *op. cit.*, p. 205, observed in a similar connection: "The advantages of provisionally ruling out all these complications by the stable-price assumption are certainly great. For there is little that a general theory can do about this mess of 'possibilities.' "



but this is not attempted here. Those investigations of capital movements which have ignored the output-generating role of the new investment need to be re-examined. This group, incidentally, includes virtually all such studies of capital movements. The theory has dealt almost exclusively with the transfer problem, and the capital inflow has been treated as if it were a pure transfer payment. When such case studies as Viner's of Canada, or Wilson's of Australia, are re-examined to permit the expansion of capacity to have an effect, the nature of the process may be made clearer. But this remains to be done.

However, it seems clear that the output-expanding character of investment needs to be incorporated into the theory of capital movements. When substantial capital imports take place, an analysis that considers only the price and income changes consequent upon the monetary transfer cannot be expected to explain the observed events.

# COMMONALITY, THE PRICE SYSTEM, AND USE OF WATER SUPPLIES<sup>1</sup>

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## I. INTRODUCTION

The almost constant stream of news reports in the press and radio regarding falling water tables, salt water intrusion, and increased water use is indicative of the growing attention being given to the subject of water supply in the United States. Water is rapidly losing its status as a "free good" and its "wise" use is becoming a problem of national concern. Unfortunately, the publicity has often emphasized the "scare" aspects of the situation and has given the impression that unless strong action is taken that the nation is in imminent danger of running out of water and thus facing a sharp curtailment of growth. It is the belief of this writer that there are indeed some important water problems but that the "scare" emphasis is misplaced and possibly ill-conceived in that it may cause the citizenry to take action not really warranted by the facts of the situation.

In addition to the possibility of misplaced emphasis, this writer is concerned with the neglect of careful economic analysis in a number of the water studies that have been made. While considerable attention has been given to the geological, engineering, and administrative aspects of water provision, relatively little attention has been paid to the economic questions involved. There is considerable evidence that the failure to use basic economic analysis has resulted in pricing and administrative policies which are at variance with economic realities and which are in conflict with the efficient allocation of water resources.

It is the thesis of this paper that greater reliance should be placed upon the price system to allocate water resources efficiently among competing uses and users. Aside from the outright failure to allow the price system to function in many instances where its use is now possible, the application of "correct" pricing policies to water resources, however, is plagued by that fact that water resources are often exploited or utilized in common so that important "spill-over" costs exist. The commonality of use prevents the onus of costs from falling upon the particular pumper and causes a divergence between private and social costs. As a result optimum allocation of water resources is hampered. I shall propose two basic methods of destroying or mitigating the worst aspects of commonality. It is my belief that, once the problem of common usage is dealt with, the price system can be used more extensively and more effectively as a regulator of the production and use of water.

## II. WATER AS A RESOURCE

Except for some cases of pre-historically stored water partaking the nature of a "water mine," water is a renewable resource. In this respect water is similar

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to other "flow" resources such as plant-life, sunlight, and wind. Precipitation in the form of rain or snow replenishes the earth's supply of water. Once the water reaches the earth it may be directly evaporated; it may take the form of surface run-off and flow to lakes, rivers, or oceans; or it may percolate through porous soil into underground basins or aquifers.

Water differs, however, from many "flow" resources in that it is storeable. It may be stored in natural reservoirs such as lakes, oceans, underground basins or ice caps, or it may be stored in man-made reservoirs. Because oceans cover more than seventy per cent of the earth's surface it is not likely that the earth will ever run "dry." In addition to its storeability water is transportable and because of this no region of the world need be "short" of water in a physical sense. It is physically possible, for example, to provide a plentiful supply for the Sahara Desert.

In the United States the average rainfall is estimated to be thirty inches annually on a total area of three million square miles. Of this amount more than seventy per cent is lost directly through evaporation or through transpiration by vegetation. The remainder, about eight and one-half inches, becomes surface run-off or percolates underground and is available for use. Thus far we have made only a very small start in the use of that amount. It is estimated that only about one inch of the eight and one-half inches is withdrawn for use by man.<sup>2</sup> In addition to the great physical possibilities of increasing our use of the total potential water supply there are untold possibilities of continued re-use and reclamation of existing supplies. Many "scare" reports designed to emphasize the seriousness of the national water problem neglect this latter point.

In Southern California, for example, where the water supply problem is usually characterized as "critical," some ninety-five per cent of all of the sewage waste in Los Angeles County is discharged into the ocean through an extensive outfall system.<sup>3</sup> Studies concerning the feasibility of reclaiming this sewage water show that it can be completely reclaimed for re-use at costs well under those of the proposed importation of water to Southern California from the Feather River in Northern California. And some sewage reclamation is now possible at costs lower than those of existing supplies. Yet, thus far, the official California water resource publications do not list sewage reclamation as a possible source of future water supply. It is the belief of this writer that failure to consider that water often can be re-used is a serious stumbling block to intelligent thinking in regard to water resource allocation. And when existing supplies are priced correctly the economic feasibility of reclamation will become even more evident.

Although studies seem to indicate that water usage in the United States has increased four fold since 1900 and that we may expect the 1975 use of water to be double the 1950 usage, there seems to be little likelihood that there will be a

<sup>2</sup> Douglas McKay, "Water: Government Plans and Policies," *Dun's Review and Modern Industry*, Vol. 65, No. 2324 (April, 1955), p. 49.

<sup>3</sup> C. E. Arnold, H. E. Hedger, and A. M. Rawn, *Report Upon the Reclamation of Water from Sewage and Industrial Wastes in Los Angeles County, California*, for the County Sanitation Districts of Los Angeles County (Los Angeles, April, 1949), p. 17.

physical shortage of water. The real problems in dealing with water will be the same as they are now: how best to use the existing supplies in any area, and secondly, how and when to expand the supply as the demand for water increases. Water as a general resource should not be considered any more "scarce" than most other kinds of natural resources. Because of its renewability, it is actually less "scarce" than many resources, such as coal, iron or petroleum. From an economic standpoint more water can always be provided at a higher price, and in this sense use of water is just one part of the general economic problem of maximizing production from scarce means. While it is true that a particular area may lack a "cheap" local water supply, it should be remembered that additional water can be supplied if it is economically feasible, i.e., if the extra returns will cover the extra costs of the extra supply.

Ideally water supplies should be allocated so that the value of the marginal product is equated in alternative uses; prices should equate the quantity demanded with the quantity supplied; and water production should be carried up to the point where the value of the marginal product equals the marginal cost.

Water allocation based upon political or administratively determined priorities such as exist in many states where, for example, agricultural users are accorded higher priorities than industrial users may well violate the basic principle that resources should be allocated to uses where the value of the marginal product is the highest. There seems to be a built-in bias that agricultural users are superior to industrial users even when the worth of the water to industry is many times that of agriculture. Administrative allocation by priorities is not an efficient substitute for allocation of resources by the price system.

To summarize, water as a resource or factor of production has no unique qualities which should cause it to be treated differently from any other economic resource. Any attempt to single out water resources for special consideration may result in error. While the institutional pattern of ownership, especially as concerned with commonality of exploitation, may call for attention, the use of water at any time and place should be considered within the framework of the general economic principles of resource allocation.

### III. COMMONALITY OF WATER PRODUCTION

The major deterrent to the increased use of the price system to allocate water resources is the fact that most water resources are exploited or utilized in common. As long as the quantity supplied far exceeded the quantity demanded the problems of common usage were not very serious or evident, but the large increase in water use in recent years has served to bring the problem of commonality to the forefront. Whether the water is pumped from a river, a lake, or from wells, the pumping usually takes place in common with many individual pumpers. "Spill-over" costs arise when all of the costs of extra pumping do not fall upon the individual pumper but are borne instead by other pumpers in the same basin and by society in general. If the production of water were not in common, but private where the incidence of costs fell solely on the individual pumper, there would be no "water problem" as distinguished from the general problem of economizing.

Actually the case of water production is closely related to the "oil-pool" case. In both situations producers are exploiting a common supply. Two major types of "spill-over" costs result from this commonality of use. The first and most serious is that each pumper at the common source of supply has no incentive to maximize the present value of total extractions over time because he has no property rights which are valid in the future. Each producer has the incentive to pump as long as the current marginal returns exceed his current marginal costs with the result that possible future values of the remaining supply are ignored. This type of "spill-over" cost is especially important when the supplies of the resource are exhaustible as in the case of non-renewable water supplies. Examples of such water mining are found throughout the High Plains area which stretches from the Black Hills of South Dakota down to the Texas Panhandle and in the South Coastal Basin of Southern California. In the High Plains area of Texas it has been estimated that the farmers are pumping nearly five million acre-feet of water a year in an area where the annual recharge is less than one-hundred thousand acre-feet annually.<sup>4</sup>

This is not to say that water mining is wrong in itself. To the contrary, as long as the interest rate is positive some degree of mining is economically desirable. But it is to assert that the present values of future returns sacrificed by present pumping are being ignored so that the tendency is to over-utilize the stored supply.

In addition to the problems involved in competitive exploitation of a common water mine there are many problems of salt-water intrusion and compaction which arise from this type of spill-over cost. In some cases a supply of water, formerly renewable, is rendered non-renewable because of the salt water intrusion (and/or compaction) which may permanently destroy the value of the ground water basin. If pumping were not in common, but private, no pumper would rationally overpump his own basin and cause it to be permanently destroyed by intrusion unless the present value of the water pumped exceeded the present value of all future water incomes destroyed—not a likely possibility. Where pumping is in common individual pumpers never consider in their cost calculations either the future values of the water or the possibility that their pumping may cause salt water intrusion in other wells nearer the sea coast.

The second type of spill-over cost is one which exists even where there is no problem of exhaustibility. It is the cost which results when the one pumper lowers the level of the pool and thus part of the cost of the extra pumping lift is then borne by all of the common pumpers. This type of spill-over cost is similar to the congestion costs created by an extra truck on an already busy highway. The extra trucker or pumper is not faced with the total cost of his action but bears only the part of it which falls on him. It is clear in this type of spill-over cost as well as in the first one that production will tend to exceed the social optimum because the decision makers do not bear all of the costs attributable to their production decisions. It is also clear that commonality is at the heart of the matter because if a producer were the sole owner and operator of a specific

<sup>4</sup> Thad G. McLaughlin, "Hydrologic Aspects of Ground Water Law," *Journal American Water Works Association*, Vol. 47, No. 5 (May, 1955), p. 449.



water resource he would, if rational, be forced to take these extra costs of extra pumping into consideration. It may be mentioned that the spill-over costs from pollution are definitely in the same category.

As is generally admitted the commonality problems in regard to the "oil-pool" case have not been satisfactorily resolved. The "law of capture" still prevails except as modified by federal and state regulation. Pumping quotas are often set with regard to protecting the market price structure of petroleum. There is little evidence to suggest that either of the two types of "spill-over" cost mentioned above are adequately dealt with. Suggestions of various students of the problem for pooling of pumping rights and operation of the pool in the manner of a sole owner through cooperative effort have met with only limited acceptance.

In regard to the commonality of water use there has been little recognition of the problem in most parts of the nation probably because of the former status of water as a "free good." In the communities of the West the need for economy of water use has forced some consideration of the problem although no entirely satisfactory solutions have been imposed as yet.

#### IV. COMMONALITY AND WATER LAW

As many water authorities have pointed out,<sup>5</sup> legal concepts concerning the use of water, particularly ground water, in most parts of the United States are based upon unsound hydrologic principles. Furthermore, from an economic standpoint, some of the legal framework is based upon the implicit assumption that water is a "free good." The English common law of riparian doctrine, which is the basis of most water law in the United States except in the West, is clearly based upon conditions of an excess of water supply in relation to demand. Each riparian owner is allowed to use as much water as needed without regard to the use by neighbors. This is particularly true in regard to percolating water where under riparian law there is no protection to a well owner from the lowering of the water table caused by his neighbor's action. And in many states each land owner along a stream is allowed to use the full flow of that stream. The worst features of commonality are present and the legal precedent is such as to encourage it.

In the western states where the doctrine of prior appropriation prevails, the matter is somewhat better. Each pumper is granted the legal right to pump water based upon prior pumping. Subsequent pumpers acquire rights which are junior to those of prior appropriators. Appropriations of waters declared to be "surplus" are usually determined on the basis of application to the state water board which may be guided by a schedule of priorities.

The economic effect of the prior appropriation law is to eliminate a large part of the commonality feature. Each user has a specific supply. As such that supply or right may be considered private so that rational calculations are possible in guiding its disposal. As water becomes more and more valuable, the owner is able to collect implicit or explicit economic rent so that the water tends to be valued at its worth in alternative uses instead of at its "cost of production." And

<sup>5</sup> Thad G. McLaughlin, *op. cit.*, p. 447; Harold E. Thomas, *The Conservation of Ground Water* (New York: McGraw-Hill Book Co., 1951), p. 243 ff.

to the extent that water rights can be bought and sold the water tends to be allocated to its most productive uses. Not all of the spill-over costs are eliminated, however, in that one man's pumping still lowers water levels to all pumpers, but it might be argued that these are relatively unimportant, compared to the allocative improvement resulting in making a common supply private.

With regard to the status of riparian water law the State of California has made significant progress in attacking the commonality problem. The California Supreme Court has developed what is usually known as the California doctrine of correlative rights. As the doctrine has developed common riparian pumpers are each accorded a "reasonable" share in cases where the supply is not sufficient for all. In the famous case of *Pasadena v. Alhambra*<sup>6</sup> the doctrine of correlative rights was used to adjudicate water claims of common pumpers in a classic commonality case where the water claims exceeded the long-run safe supply. Each party was restricted to a pro-rata reduction in the quantity of water he had been pumping, and the total pumping was limited to the safe yield of the basin. In effect, the court by specifying definite water shares, removed one of the major aspects of commonality. Each user is prevented from taking action which might pre-empt his neighbor's supply or in causing salt water intrusion on the coast. Here, as in the use of prior appropriation, not all spill-over costs are eliminated but certainly a large advance has been made.

#### V. TWO METHODS OF ATTACKING THE PROBLEM OF COMMONALITY

It is evident that the problem of common usage of water supplies must be dealt with before efficient utilization of water resources can be accomplished. As long as there are serious divergences between private and social costs the extended use of the price system will be hampered. The establishment of priorities to use and of legal prohibitions can serve only as stop-gap measures until the central features of commonality are mitigated or destroyed.

This writer believes that there are two major lines of attack for dealing with the commonality problem. One lies in the adjudication of specific water rights through the setting of titles as in prior appropriation law or in the pro-rating of water use to riparian users as in the case of *Pasadena v. Alhambra*. The second method is the imposition of extraction or "use" taxes designed to correct the divergence between private and social costs. These two methods offer strikingly different ways of dealing with common usage but both would destroy some of the central features of commonality so that more rational pricing policies could be followed to assist in optimal resource allocation. I shall refer to the two methods as the pro-rata and the "use" tax solutions.

#### VI. THE PRO-RATA METHOD

The pro-rata method is one which attacks the problem of common usage by adjudicating quotas and by making water supplies separable or specific. This solution appears to be most feasible in cases where the supply is largely of a renewable nature, i.e., where the supply is replenished and where the hold-over

<sup>6</sup> *Pasadena v. Alhambra* 33 Calif. (2d) 908, 207 Pac. (2d) 17 (1949).



storage from year to year is relatively small so that total pumping would be limited to the safe yield of the water system in question. If the main spill-over costs had been those principally of salt water intrusion and the failure of economic rent to accrue because commonality limited water prices to those of the "cost of production," the pro-rata method would be satisfactory. It would not eliminate the type of spill-over cost of higher lifts on all pumpers caused by the action of one pumper. In most cases, however, these would be relatively unimportant.

Actually the "spill-over" costs of higher pumping lifts remaining in the pro-rata solution may not be relevant from the viewpoint of society. The price of water would be such as to equate the demand with the fixed total supply of the system. Once economic rents accrue these "spill-over" costs would no longer affect the price of water. They would merely affect the relative cost of production as among pumpers and therefore would result only in changing the distribution of the economic rent *between* individual pumpers but not its total.

In instances where the annual recharge to the system is small in relation to the total water shortage, it is evident that the restriction of total pumping to the amount of recharge would not be economic. In this case mining of the water would be economically desirable as long as pumping in excess of recharge would not invite salt water intrusion or compaction. Here the total amount to be mined would be the relevant consideration so that the quotas would have to apportion the water in storage as well as the recharge. If the amount to be mined were relatively large and if the amount mined in the present seriously affected the costs of mining in the future, it is clear that the pro-rata solution would not be very satisfactory. Each owner would have title to a specific portion of the fixed common supply, but future values would be taken into consideration only to the extent that the costs of higher lifts caused by a neighbor's pumping were less than the present values of the expected future returns.

It is obvious that adjudication of quotas will be most satisfactory where the amount of water mining is small in relation to recharge or where most of the mining that is economically feasible has already taken place. From a practical standpoint this will often be the case as pressure for adjudication will not often be present until much of the large hold-over supply is exhausted.

It has been argued that rights which evolve in such adjudication procedures are arbitrary in that they may depend upon historical considerations and that they may lead to waste.<sup>7</sup> It is the belief of this writer that such charges need to be carefully thought through. As to the first point that pro-rating may be based upon historical use, it may be pointed out that such is a common feature of almost all private property rights. The fact that pumpers may be establish claims to water supplies by historical use is somewhat similar to the action of early settlers acquiring land, mineral, and other rights which later proved to be valuable. In all of these cases our society has seen fit to let the economic rent accrue to the

<sup>7</sup> *A Brief Survey of the Technology and Economics of Water Supply*, by James C. DeHaven, Linn A. Gore, and Jack Hirschleifer, RAND Corporation, Report R-258-RC (Los Angeles, October, 1953), p. 40.

owners even though the acquisition of the property may have been a pure historical accident. If one is to argue that historical use of water should not be used to establish title to the resource, he should be prepared to show why water should be treated differently from other natural resources acquired in a like manner.

On the second point it was argued that: "pro-rating offers no bar at all to waste of water until one's allotment is reached, and thereafter permits no use whatsoever, however intense or well justified the need."<sup>8</sup> It is the opinion of this writer that such a charge is based upon the implicit assumption that individual entrepreneurs tend to be irrational and cannot be depended upon to use their own property in an "efficient" manner. If one is to argue that private owners will waste their own water, he must be assuming that they are irrational. Presumably, a rational owner knows the value of his water in alternative uses. Because water rights can be bought and sold and because water itself is transportable and storable, each quota owner would have incentive to consider the value of his right (and his water) to alternative users. The price of water would be high enough to equate the total supply with the total demand, and each quota owner would be able to earn explicit economic rent depending upon the strength of demand in relation to the fixed supply. Persons without rights but with a great need for water could well afford to pay dearly for a water right. Each owner of a quota would always have the choice of the sale of his right (or the sale of his water) and the ordinary market mechanism would serve to allocate the scarce supply. To deny rational use of one's own quota is to question whether private individuals can make rational decisions in the utilization of all other property at their disposal, not just water. Pro-rating by adjudication may well present difficulties, but the charges that historical use is arbitrary and that wasteful use need result should not go unquestioned.

#### VII. THE "USE" TAX METHOD

The other alternative method of dealing with the problem of commonality of water use is to levy a tax upon all pumping so that the tax plus the private pumping costs would be equal to all of the extra costs of the extra pumping. This method is the famous classical solution to correct a divergence between private and social costs where the social costs are greater as stated by Pigou.<sup>9</sup>

To illustrate the nature of the "use" tax we shall refer to Figure 1 below.

The marginal private cost is just the cost of pumping to individual pumpers. The marginal social cost measures the losses in productivity to the community at large because of private pumping, including higher pumping lifts and the possibility of salt water intrusion. In addition the marginal social cost should include an estimate of the present values of future incomes sacrificed if water mining is possible. It is clear that the optimum output would be OM and that the "use" tax should be AB or ED in order to make the price of water equal to OA. Under competitive exploitation where commonality is present the output

<sup>8</sup> *Loc. cit.*

<sup>9</sup> A. C. Pigou, *The Economics of Welfare* (London: Macmillan and Co., 4th ed., 1932), p. 192.

would be larger than optimum or ON and water would tend to be priced only at OC.

It may be noted that the total tax revenue collected by the state would be ABDE while the total social costs would be only FDE if a per unit tax of ED is levied. It is clear that the excess of tax revenue above the total social costs would involve a transfer of economic rent from the private pumpers to the state. If this is thought undesirable, it might be possible for the state to return tax proceeds in excess of FDE to the pumpers in the form of "dividends" not related to the amount pumped.

Actually, the optimum tax structure appears not to be the levy of a fixed tax per unit but a series of graduated taxes designed to make the total tax revenue equal to the total social cost. A tax of ED would be levied only on the marginal unit while the infra-marginal units would be taxed at rates equalling the difference between marginal social and marginal private costs. In this case economic rent in excess of the social costs would remain with the pumpers. For administrative feasibility a series of block tax rates could be devised as a practical substitute for collecting the exact difference between marginal social and marginal private costs on every unit of output.

To view the problem of pro-rating of quotas within this same context it is clear that OM should be the optimum quota. The price of water would also be OA but all of the economic rent and all of the revenues representing social costs would accrue to the private quota owners. In addition the expensive adjudication process would have to be reinstituted with likely changes in the value of marginal product and marginal cost curves. Of course, the "use" tax would need to be changed also in similar circumstances but that would be much easier administratively.

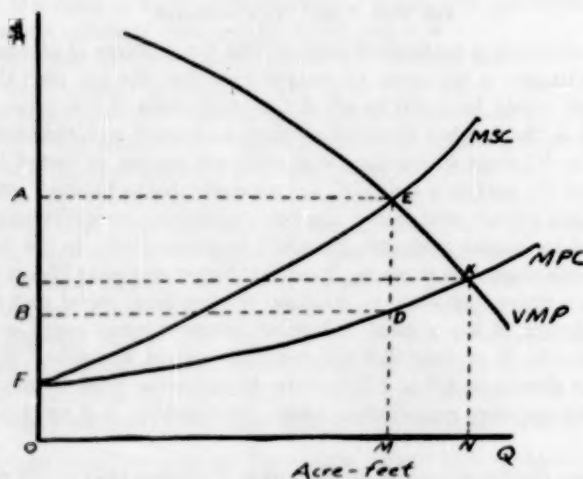


FIG. 1

The pro-rata method would work much better in the case where total pumping should be limited to long-run recharge. In that case the marginal social cost curve would become irrelevant at the output corresponding to the long-run safe yield and the relevant consideration would be the intersection of the value of the marginal product curve with the fixed supply. The advantage of quotas here is that once established they could remain fixed unless our knowledge of the long-run safe yield changed. All economic rent would accrue to the private owners. The use-tax method would appear to be administratively clumsy in the fixed supply case as the tax would continually have to be adjusted with every change in the value of the marginal product or with changes in private pumping costs. It is obvious that all economic rent could accrue to the state.

#### VIII. APPRAISAL OF THE TWO METHODS

From an analytical standpoint the two methods of dealing with commonality of water use offer very different lines of attack. Both solutions deal with the problems of the divergence of private and social costs by destroying the central features of commonality. Pro-rating imposes private title to a specific share of the common supply. "Use" taxes place the government in the position of a "sole owner." The state by levying the tax is actually selling rights to use or exploit the resource. In effect the management of the resource is transferred to the state. In contrast to the pro-rata method most or all of the economic rent would accrue to the state instead of to private quota owners. No "waste" would result in that each pumper would have to pay a price equal to all costs, social and private. And water would be automatically allocated according to the workings of the price system as it reflected market forces and the water would go to uses where it could be used most productively.

By contrast the pro-rata method allows the economic rent to accrue to private owners. Here also the price system would serve to allocate scarce water resources. As the demand for water increases each owner would have the incentive to consider the value of his water to alternative users. Rational consideration would call for the collection of explicit economic rent or the imputation of implicit economic rent. The effectiveness of the pro-rata method would depend to a great extent on the market structure. It would need a competitive market for water resource to obtain the best results. The "use" tax method, however, could be imposed upon a monopsonistic market and yield excellent results. The "use" tax method would seem to be superior where water mining is economically feasible and the pro-rata method would appear to be desirable where total pumping should be limited to long run recharge.

Both solutions require basically the same information for their establishment and both possess inherent administrative difficulties. Pro-rating would require a lengthy and expensive adjudication process and reliance upon "arbitrary" standards to determine quotas. Once the quotas are determined, however, no further governmental action is required, except as the knowledge of the "safe yield" changes. The "use" tax appears easier to establish initially but the size of the tax would have to be constantly readjusted as the demand and supply

conditions changed. In actuality we would be satisfied with a rough approximation in setting the tax. In addition there is the problem of how the state should dispose of the economic rent collected by the taxes.

Aside from the problems of water mining and those of market structure, the choice of which method to use in dealing with the problem of commonality of water use seems to depend principally upon the social and institutional considerations involved. For example, in reference to a river or a lake the "use" tax method may be preferable if these water sources have long been considered "public" anyway. For underground waters, where overlying owners have long considered the pumping of water to be "private" in nature, the pro-rata method would probably be preferable. Much depends upon whether there is substantial legal basis upon which to base either a "use" tax or an adjudication process.

An additional feature of the "use" tax method is that it would be very easy to establish a system under which consumptive-use pricing of water could be followed. That is to say, rebates could be instituted for water not "used up" in the productive process which is returned to the lake, river or ground water system in question. Rebates could also be devised which would vary with the degree of contamination of the water returned. Such incentives would go far in encouraging industry to conserve in its use of water and help prevent serious pollution problems.

In the pro-rata method much the same incentives would be present with regard to consumptive use. Re-use of the water would continue as long as the costs of re-use were less than the cost of additional supplies. The problem of pollution of a neighbor's well or the case of disposal of waste into a stream would be difficult to deal with by the establishment of quotas alone and may well call for complementary use taxes. While each of these methods have been viewed in isolation for analytical purposes, there is no reason why they may not be combined.

It will be noticed that neither solution, as set forth above, deals effectively with the problem of the proper *scale of operation* in use of pumping resources. It may be the case that the same amount of water, e.g., the "safe yield" of the basin system, could be pumped cheaper or with less equipment if the operation of the wells were centralized and not left in individual hands. The spacing of the wells, the location of the well, and the type and size of the pumps, all may be important in determining the optimum or least cost scale of pumping.

If these returns to scale were judged to be significant, two courses of action could be taken. Under the pro-rata method, with its reliance on private resource decisions, it could be possible for the quota owners to consolidate their water rights to form cooperatives and centralize decision making so that the proper scale of pumping could be chosen. Or it would be possible for one owner to buy rights of other owners to obtain a total allotment consistent with the optimum size of the plant.

If we rely on the use tax method in which the state is, in effect, the "sole owner," the course of action to take advantage of economies of size is clear. The state could either buy out and take over all wells and run them as a "sole



operator," or, alternatively the public authorities could decide on the proper number and location of wells and license private owners to construct and operate them.

#### IX. CONCLUSIONS

(1) The economic problem in the provision of water is to make sure that new increments of supply are provided only when their development reflects the intensity of the need for water in relation to the alternative uses of the resources involved. There is danger that the fear of a water shortage may obscure the fact that water production is merely a part of the general problem of resource allocation, and that we must insure that the extra returns justify the extra costs of obtaining the additional water supplies.

(2) Most water supplies are exploited in common. The commonality of use prevents the burden of costs from falling upon the particular pumper and causes a divergence of private and social costs. Furthermore, commonality prevents the acquisition of property rights to water so that water tends to be valued at its "cost of production" instead of at its economic worth and economic rents fail to appear.

(3) Two solutions, which would destroy or mitigate commonality, are possible. The establishment of adjudicated quotas could prevent overdraft and salt water intrusion and assist in the rational valuation of supplies on the part of private users. The imposition of "use" taxes to correct the divergence between private and social costs, the "classical" solution, is the second alternative. Each method has its own merits and more discussion of each is called for.

(4) It is essential that the commonality problem be dealt with so that the price system can aid in the optimum allocation of water resources. Administration allocation by priorities is not a satisfactory substitute.

# A CASH-BALANCE INTERPRETATION OF DEPRESSION

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## I. THE CASH-BALANCE APPROACH

The usual account of inflation or depression stresses too much or too little demand for goods and services. It is enlightening to reverse this emphasis by focussing on the demand for and supply of money. The present paper views depression as an excess demand for money, in the sense that people want to hold more money than exists. It views an inflationary boom as an excess supply of money, in the sense that more money exists than people want to hold.

This interpretation has advantages:

1. It provides a unifying framework into which various strands of theory—the saving-investment relation, the alleged Keynesian underemployment equilibrium, the Pigou effect, an interpretation of Say's Law, a clarification of the terms "inflation" and "deflation," and the relation between price levels and production-and-employment levels—fit neatly as special aspects. It avoids some pitfalls of partial-equilibrium analysis of individual markets by focussing on the one thing—money—exchanged on all markets.

2. The cash-balance approach achieves this unity by tying macro- and micro-economics together, by handling depression and inflation with the familiar concepts of supply of and demand for a particular thing. In focussing on the cash-balance decisions of individual firms and households, it draws on a leading source of empirical generalizations in economics—economists' "inside" knowledge of human motives and decision-making.

3. Viewed as dealing with imbalance between the demand for and supply of money, business-cycle theory sheds some ambitions tending to lead it astray. Actually, there is no more reason to search for one universally valid explanation of such imbalance than there is to search for one universally valid explanation of an excess demand for or excess supply of any ordinary commodity—or than to search for a one-and-only cause of broken legs.

4. An account of the relation between the total money stock and people's efforts to build up, cut, or maintain their cash balances can be presented as a logical translation of the more familiar effective-demand and saving-and-investment theories. Thus nonmonetary theorists will have a hard time showing that the cash-balance approach is wrong, even though they may object to its dragging hidden assumptions about money out into the open.<sup>1</sup>

\* The author thanks Mr. Norman Lombard and Professor Dudley Dillard for helpful comments on a much earlier and longer draft.

<sup>1</sup> Even ostensibly "nonmonetary" business-cycle theories must, at least tacitly, allow changes in the flow of money. Cf. D. Hamberg, *Business Cycles* (New York, 1951), pp. 193, 216, 217, 220, 372. As Hamberg says on pages 113-114 and as Gottfried Haberler says in *Prosperity and Depression*, 3rd ed. (Geneva, 1941), p. 101, the acceleration principle cannot dominate the whole economic system rather than just particular sectors unless the money or credit supply is elastic.



5. The cash-balance approach helps distinguish between treatment of unemployment due to general deficiency of effective demand and treatment of unemployment due to other troubles. It shows how a policy of price-level stabilization through monetary and fiscal action would coincide with preventing unemployment of the first type while not misusing expansion of demand as an inappropriate weapon against unemployment of the second type.

6. The cash-balance approach need not, surprisingly, presuppose any precise dividing line between money and near-moneys.

## II. SAY'S LAW AND MONEY

Say's Law, or a crude version of it, rules out general overproduction: an excess supply of some things in relation to the demand for them necessarily constitutes an excess demand for some other things in relation to their supply.<sup>2</sup> This seems an unassailable truism. Apparent overproduction in some industries shows not general overproduction but only disharmony between the relative outputs of various industries and the pattern of consumers' and investors' preferences. Subnormal profit opportunities in some industries must be matched by above-normal profit opportunities elsewhere. *General* depression is impossible.

The catch is this: While an excess supply of some things does necessarily mean an excess demand for others, those other things may, unhappily, be money. If so, depression in some industries no longer entails boom in others. Say's Law assumed a peculiar kind of demand for money: people, taken together, were always satisfied with the existing quantity of money and never wanted to change their total cash balances except to adapt them passively to changes in the total quantity of money available.<sup>3</sup>

Actually, the quantity of money people desire to hold does not always just equal the quantity they possess. Equality of the two is an equilibrium condition, not an identity. Only in what Oscar Lange calls *monetary equilibrium*<sup>4</sup> are they equal. Only then are the total values of goods and labor supplied and demanded equal, so that a deficient demand for some kinds entails an excess demand for others.

Say's Law overlooked monetary disequilibrium. If people on the whole are trying to add more money to their total cash balances than is being added to the total money stock (or are trying to maintain their cash balances when the money stock is shrinking), they are trying to sell more goods and labor than are being bought. If people on the whole are unwilling to add as much money to their total cash balances as is being added to the total money stock (or are trying to reduce their cash balances when the money stock is not shrinking), they are trying to buy more goods and labor than are being offered.

The most striking characteristic of depression is not overproduction of some things and underproduction of others but, rather, a general "buyers' market,"

<sup>2</sup> Oscar Lange, "Say's Law: A Restatement and Criticism," in O. Lange and others, eds., *Studies in Mathematical Economics and Econometrics* (In Memory of Henry Schultz) (Chicago, 1942), pp. 49, 53, 57-58.

<sup>3</sup> *Ibid.*, p. 53.

<sup>4</sup> *Ibid.*, p. 52.

in which sellers have special trouble finding people willing to pay money for goods and labor. Even a slight depression shows itself in the price and output statistics of a wide range of consumer-goods and investment-goods industries. Clearly some very general imbalance must exist, involving the one thing—money—traded on *all* markets. In inflation, an opposite kind of monetary imbalance is even more obvious.

### III. DEMAND FOR AND SUPPLY OF MONEY

Whether we regard the quantities of money supplied and demanded as stocks or as flows is a matter of convenience rather than of principle.<sup>5</sup> Equilibrium in the stock sense coincides with equilibrium in the flow sense. When people on the whole want to hold exactly the quantity of money in existence, they cannot be wanting to change their cash balances at a rate different from the rate at which this quantity is changing. Similarly, disequilibria in the stock and flow senses coincide. People on the whole cannot keep on trying to adjust their cash balances to equal more or less than the total money supply unless they are at the same time trying to change their cash balances at a rate different from the actual rate of change in the money supply. That is, if people demand cash balances totalling more or less at some particular instant than the existing money supply, then the demanded rate of change in cash balances is infinite (a finite change in zero time). The demanded infinite rate of change in cash balances cannot be equal to any actual rate of change in the money supply.<sup>6</sup>

Households and businesses demand cash balances for what are usually classified as transactions, precautionary, speculative, and investment motives.<sup>7</sup> Consideration of these motives shows that the total of cash balances demanded tends to be positively associated with the physical volume of transactions paid for in money (which depends in turn on payment practices and other institutional conditions, on the human and business population, and on the level of production or real income) and with the level of prices and wages. Interest rates and expectations of future price levels and business conditions also presumably have some effect on the demand for money. The supply of money can conveniently be regarded at any one moment as a definite quantity, which government and banking operations change over time.

As just implied, the number of money units that people demand to hold in their cash balances varies inversely with the purchasing power, or value, of the unit. (A person wants to hold fewer dollars in America than francs in France.) The similarity between the demands for money and for any ordinary commodity is clear.

For any ordinary commodity, there is some price at which the amounts de-

<sup>5</sup> Edwin Cannan, "The Application of the Theoretical Apparatus of Supply and Demand to Units of Currency," *Economic Journal*, Dec. 1921, XXXI, pp. 453-54.

<sup>6</sup> For another demonstration that excess demand for and excess supply of money in the flow sense coincide respectively with excess demand and excess supply in the stock sense, see Don Patinkin, "The Indeterminacy of Absolute Prices in Classical Economic Theory," *Econometrica*, Jan. 1949, XVII, pp. 5, 7-9.

<sup>7</sup> Albert Gailord Hart, *Money, Debt and Economic Activity* (New York, 1948), pp. 195-208, 523-25.

manded and supplied would be equal. And so with money: there is some value of the money unit that would equate the amounts demanded and supplied. But—again as is true of any ordinary commodity—the equilibrium value at one particular time might be a disequilibrium value later. Supply and demand schedules are always shifting.

Since the prices of many goods and services are notoriously "sticky," the value of money does not adjust readily enough to keep the amounts of money supplied and demanded always equal as schedules shift. The value of money is often "wrong." Depression is such a disequilibrium: given the existing levels of prices, wages, and interest rates, people are on balance more eager to get money by selling goods and labor than to give up money in buying goods and labor.

This interpretation harmonizes with the Keynesian theory, which attributes a cyclical fall in income to an excess of intended saving over intended investment. The very fact of oversaving implies the existence of some form other than goods in which people can accumulate savings: if people are trying to save more money than they or others are willing to spend on "real" investment, people on the whole must be trying to acquire larger cash balances than are available in the aggregate. Conversely, if people are trying to spend more money on "real" investment than they or others are willing to save, then people on the whole are trying in vain to reduce their cash balances. (Or, if the money supply is growing, people are demanding additions to their cash balances that are smaller than the additions to the money supply.) It follows that an excess of intended saving over intended investment is an excess demand for money and that an excess of intended investment over intended saving is an excess supply of money.

Decisions about saving and investment are largely decisions about the holding of cash balances. Some factors affecting businessmen's willingness to make investments—price expectations and the state of business "confidence," for example—coincide with factors affecting the amounts of money that businessmen wish to hold. Keynes himself devotes chapter 17 of his *General Theory* to an analysis of the "essential properties" of money which at times make people prefer so strongly to hold money rather than capital goods that investment is insufficient. He explains that the liquidity-premium and low carrying cost of money may keep the demand for it from being readily choked off, that the money supply is inexpandable in a depression (apart from official action), and that the elasticity of substitution of other assets for money is slight. Keynes continues:

The first condition means that demand may be predominantly directed to money, the second that when this occurs labour cannot be employed in producing more money, and the third that there is no mitigation at any point through some other factor being capable, if it is sufficiently cheap, of doing money's duty equally well. The only relief—apart from changes in the marginal efficiency of capital—can come (so long as the propensity towards liquidity is unchanged) from an increase in the quantity of money, or—which is formally the same thing—a rise in the value of money which enables a given quantity to provide increased money-services.

Unemployment develops, that is to say, because people want the moon;—men cannot be employed when the object of desire (*i.e.* money) is something which cannot be produced and the demand for which cannot be readily choked off. There is no remedy but to persuade the

public that green cheese is practically the same thing and to have a green cheese factory (i.e. a central bank) under public control.<sup>9</sup>

In the Keynesian theory, intended saving and intended investment are made equal by fluctuations not so much in interest rates as in income. Excess intended saving cuts income until intended saving falls to the level of intended investment. The cash-balance theory accounts for something equivalent. Excess demand for money means deficient demand for goods and labor, which brings on cutbacks in production and employment. The resulting drop in income reduces the demand for cash balances on account of the transactions motive<sup>10</sup> and probably on account of other motives also. When poverty had cut the total quantity of money demanded down to the quantity in existence, it would no longer be strictly correct, I suppose, to speak of an excess demand for money. The excess demand would be virtual, not actual. Poverty would be suppressing it. The situation would correspond to the somewhat misnamed Keynesian "underemployment equilibrium," in which excess intended saving is being suppressed by the low level of income.

In this situation, any monetary expansion would begin to replace poverty as the means of working off an actual excess demand for money. So would any fall or further fall in prices and wages—at least, so says the theory of the Pigou effect.<sup>11</sup> While stickily falling prices and wages are a symptom of an excess demand for money, a sufficient fall in prices and wages would be a cure. Homeopathy could conceivably work. A rise in the value of money would tend to cut the number of money units demanded and so stimulate spending. Whether reliance on the Pigou effect is a *practical* road out of depression, however, requires some comment later.

The concept of stickiness in the value of money as an obstacle to restoring monetary equilibrium brings out a direct contrast between depression and suppressed inflation. A. P. Lerner has emphasized this contrast by renaming suppressed inflation "suppression."<sup>12</sup> Suppression is the condition of a "sellers' market," general shortages, and impairment of allocation by prices that develops when prices are kept from fully adjusting to monetary inflation. Depression is the opposite condition that develops when prices are kept from fully adjusting

<sup>9</sup> John Maynard Keynes, *The General Theory of Employment, Interest and Money* (New York, 1936), pp. 234-35. For an enlightening interpretation of Keynes' chapter 17, see Abba P. Lerner, "The Essential Properties of Interest and Money," *Quarterly Journal of Economics*, May 1952, LXVI, pp. 172-193.

<sup>10</sup> Emil K $\ddot{u}$ ng, *Die Selbstregulierung der Zahlungsbilanz* (St. Gallen, 1948), pp. 50-51. J. M. Keynes also recognized that a drop in income would lessen the quantity of money demanded on account of the transactions motive. However, his main emphasis (which to my mind is mistaken) was on how this effect lowers the interest rate and so stimulates investment. "The General Theory of Employment," *Quarterly Journal of Economics*, Feb. 1937, LI, p. 218.

<sup>11</sup> See, for example, A. C. Pigou, "Economic Progress in a Stable Environment," *Economica*, Aug. 1947, n.s. XIV, pp. 180-88. Surprisingly, Keynes himself hints at the Pigou effect in his passages quoted above.

<sup>12</sup> "The Inflationary Process: Some Theoretical Aspects," *Review of Economic Statistics*, Aug. 1949, XXXI, p. 195.

to monetary deflation. As Lerner shrewdly remarks, depression is the name for (monetary) deflation with prices kept from falling.

Now we can understand the paradox that either "deflation" or "inflation" would cure depression, and that either "inflation" or "deflation" would cure suppression. The kind of deflation that would cure depression is *price-and-wage* deflation—a big enough rise in the value of money to cut the quantity of money demanded down to the quantity in existence. The kind of inflation that would cure depression is *monetary* inflation—a big enough increase in the money supply (or fall in the demand schedule for cash balances) to relieve the excess demand.

The kind of inflation that would cure suppression is *price-and-wage* inflation—a big enough fall in the value of money to raise the quantity of money demanded up to the quantity in existence. Here is the sense in the quip that the best cure for (suppressed) inflation is inflation. The kind of deflation that would cure suppression is monetary deflation—a big enough cut in the money supply (or rise in the demand schedule for cash balances) to wipe out the excess supply. (Confusion between price-and-wage and monetary "inflation" and "deflation" has sometimes bedeviled theory and policy. NRA, with its price-raising codes of "fair competition," seems to have been an example. In the absence of sufficient monetary inflation, price-and-wage deflation is a better treatment for depression than price-and-wage inflation.)

One more paradox is now understandable. Depression could conceivably be prevented either by maintaining wages and prices or—barring transitional difficulties—by cutting wages and prices. Wage-price maintenance would be salutary only if accomplished by just enough monetary expansion to avoid an excess demand for money and the symptomatic sticky sag in wages and prices. But barring monetary action, swift reduction of wages and prices to a new equilibrium level would be needed to forestall the excess demand for money that, as we are supposing, would otherwise persist.

Returning to the question whether the Pigou effect is a practical depression cure, we must first note the problem posed by a money supply made up mainly of private debt. Encouragement to money holders through a rise in the real value of their cash balances would be largely offset by discouragement to private money issuers, even though the existence of some commodity money or government-issued money suffices, in principle, for the Pigou effect to work.<sup>12</sup> A second difficulty stems from perverse shrinkage of the money supply, so well emphasized by advocates of 100 per cent reserve banking. Third, prices and wages will not in practice go down readily enough for a prompt Pigou effect; and besides, since prices and wages are not all equally flexible or inflexible, a major change in their general level would distort the structure of *relative* prices and so transitionally worsen maladjustments in production and trade. Fourth, a sticky downward sag of prices and wages would cause expectations that worsened the excess demand for money in the meanwhile. Fifth, even if prices and wages could somehow fall

<sup>12</sup> Don Patinkin, "Price Flexibility and Full Employment," *American Economic Review*, Sept. 1948, XXXVIII, pp. 547-52. Patinkin stresses also the rise in the real value of government securities.



suddenly and completely enough to forestall such expectations, the increased real burden of carrying and repaying outstanding debt would discourage business and consumer debtors. (Defaults and so forth would rule out offsetting benefits to creditors, as distinct from holders of actual money.) Sixth, such a rapid change in the purchasing power of money would subvert money's usefulness as a standard of value. Seventh, inertia would add to transitional difficulties. A person's cash balance is partly a matter of habit and is not adjusted fully and promptly to changes in the value of the money unit. When prices are falling rapidly, people may for a while thus unintentionally hold more purchasing power than usual in money.<sup>13</sup> Finally, fears of default by customers and of demands for early repayment of borrowings, together with worsened chances of borrowing in case of need, tend to increase businessmen's precautionary demands for cash balances when prices are falling. Banks, also, take customers' defaults, bankruptcies, and cash withdrawals as warnings to build up their own liquidity by reducing loans and investments.<sup>14</sup> Even households have reasons for trying to strengthen their cash positions.

Despite all these obstacles, monetary equilibrium would theoretically be restored in the long run at a new and higher value of the money unit; but "in the long run. . ."

The impracticality of waiting for a rise in the value of money to cure an excess demand for it in no way impairs our interpretation of depression as just such an excess demand. Certainly it does not discredit the idea of deliberately managing money to keep its supply and demand always in equilibrium.

#### IV. NEAR-MONEYS

One worry about the cash-balance interpretation of depression arises at first sight. Demand for current output might conceivably be slack in a depression because people preferred to hold liquid assets in general rather than actual money in particular. For instance, could not depression consist in an excess demand for bonds rather than for actual money? No: an excess demand for bonds (or for short-term bills, savings accounts, savings and loan shares, and other interest-bearing obligations to pay money) cannot persist unaccompanied by an excess demand for money itself. Given the prevailing prices, wages, and interest rates, the total value of goods and services that people want to exchange for bonds, directly or indirectly, will not exceed the total value of bonds that people want to exchange for goods and services—that is, people will not want to hold more bonds than exist—unless they also want to hold more money than exists.

The reason can be made clear by supposing, for the sake of argument, that people's preferences do shift away from goods and services and in favor of bonds without also shifting away from goods and services in favor of money. That would mean a shift toward bonds in people's preferences as between bonds and money, which would tend to raise the money prices of outstanding bonds. Bond

<sup>13</sup> In times of inflation, a comparable inertia may worsen the excess supply of money by delaying one's decision to increase one's cash balance. James Harvey Rogers, *The Process of Inflation in France, 1914-1927* (New York, 1929), pp. 132, 134, 318-20.

<sup>14</sup> Hamberg, *Business Cycles*, pp. 140, 183, 389.

prices—that is, interest rates—would adjust so as to maintain equilibrium between the desire to hold bonds and the desire to hold money and so prevent an excess demand for bonds relative to goods and services from existing in the absence of a similar excess demand for money. (Bond prices would so rise unless official intervention prevented it. If transactions at prices above the legal maximum were simply forbidden, this very prevention of equilibrium bond prices would be the straightforward explanation of any excess demand for bonds. Such a case would not show that business depressions are typified by an excess demand for bonds but not for money. If, on the other hand, the government used open-market sales to keep bond prices from rising, that very addition to the bond supply and subtraction from the money supply would prevent an excess demand for bonds relative to money and so prevent an excess demand for bonds relative to goods and services in the absence of an excess demand for money relative to goods and services.)

Furthermore, as Hicks's theory of the cost, bother, and risk of security transactions<sup>15</sup> and Keynes's liquidity-preference theory explain, there is some floor below which the interest rate on any particular kind of debt will not go. At this floor rate, the reward for holding bonds is so small that people no longer prefer to hold additional wealth as bonds rather than as cash. Any further strengthening of desires to refrain from buying current output and instead to hold liquid assets must increase the excess demand for actual money along with—or even instead of—the excess demand for bonds.

A rephrasing of this complicated argument is in order. Even if the deficient spending on current output that constitutes a depression is due to an excess demand relative to goods and services for money-plus-bonds rather than for money alone, we may properly focus attention on the excess demand for money. Whatever else may characterize it, a depression must involve an excess demand for money; an excess demand for bonds could not exist alone. People could not behave in a way that would tend (barring price stickiness) to raise the purchasing power of the dollars in which bonds are expressed and yet not also tend to raise the purchasing power of the dollars in which checking accounts and currency are expressed. Furthermore, money is a very good substitute for bonds in satisfying the demand for liquid assets. When bond prices have been bid up to where bonds yield no more interest than the floor rate explained by Keynes and Hicks, then money proper is a perfect substitute for bonds. Anything that would tend to relieve the excess demand for money proper would also tend to relieve the excess demand for liquid assets in general and so would tend to relieve the deficiency in spending on current output.

Even more obviously, depression is not an excess demand for shares of stock in preference to both bonds and money as well as to current output. Actually, the demand for stocks depends on profit or dividend prospects, which are poorer than usual in depression. If depression were an excess demand for stocks and not for money, then the money prices of stocks would tend to rise. This, of course, is the reverse of what actually happens in depressions.

<sup>15</sup> J. R. Hicks, *Value and Capital*, 2nd ed. (Oxford, 1946), pp. 163-67.

Depression certainly cannot be explained as an excess demand for nonreproducible assets in preference to current output. We know that depressions are not characterized by special eagerness to acquire Old Masters and the like.

In summary, the argument still stands that depression is an excess demand for money in preference to current output. The cash-balance interpretation does not depend on any clear dividing line between money and near-moneys. If there is an excess demand for money as broadly defined, there must also be an excess demand for money as narrowly defined.

#### V. POLICY

We have interpreted changes in the general price level as symptoms of the excess demand for money that constitutes a depression and of the excess supply of money that constitutes an inflationary sellers' market. The symptom tends in the long run to be a cure, but only imperfectly. Money management to prevent the symptom would coincide with management to prevent the disease.

To clinch our understanding of this point, let us visualize a graph measuring the volume of cash balances demanded and supplied along the x-axis and an index of the purchasing power of money along the y-axis. For familiar reasons, the curve showing the demand for money slopes downward from left to right. The supply curve can be regarded as a vertical line. Now, if either schedule shifted in such a way as to cause an excess supply of money at the old level of money's purchasing power, there would be a tendency for the purchasing power to fall. An opposite shift would tend to make the purchasing power rise. Such changes in the value of money would work towards a new equilibrium, but, as explained near the end of section III, only after delay and transitional troubles. If, however, monetary policy always kept adjusting the money supply so as to keep the supply-and-demand intersection at the same level, the value of money would not tend to change. Clearly, then, stability in the value of money is a criterion for continued equality between the quantities of money supplied and demanded. A policy of stabilizing the value of money apparently coincides pretty well with avoiding depressions and inflationary booms.

It does not coincide, however, with a guarantee of permanent full employment. Not all unemployment is due to a general deficiency of effective demand. Some "frictional" unemployment is normal. "Structural" unemployment might prevail if technology and the pattern of consumer demand required use of various factors of production in fairly rigid proportions: if the factors were in fact available in other proportions, some would unavoidably be in excess supply.<sup>18</sup> More plausibly, perhaps, price and wage rigidities might block attainment of the relative price structure needed to make businessmen and consumers choose the production techniques and consumption patterns compatible with full employment. A related difficulty could arise if an autonomous upward push on wages

<sup>18</sup> Masao Fukuoka, "Full Employment and Constant Coefficients of Production," *Quarterly Journal of Economics*, Feb. 1955, LXIX, pp. 23-44. For a broader discussion of nonmonetary unemployment, see Lloyd W. Mints, *Monetary Policy for a Competitive Society* (New York, 1950), pp. 15-28.

and prices (by union pressure, for instance) kept tending to make the existing money supply inadequate for a full-employment level of business activity. The question would arise whether to "support" a creeping inflation of wages and prices by continually expanding the money supply.

The cash-balance approach, with its emphasis on price-level movements as symptoms of excess demand for or supply of money, makes it clear why money management aimed at price-level stability coincides with preventing unemployment due to general lack of effective demand while not overdoing monetary expansion in a futile attempt to cure the kinds of unemployment that require other treatment.

A possible objection to monetary stabilization is that price-level changes could be measured in many different ways; nobody could say just how much the value of money had changed over a certain period, or even, perhaps, whether the value had gone up or down or held steady. Granting all this, there is still a great difference between a clear change in the value of money as shown by *any* reasonable indicator and, on the other hand, real doubt whether the value had risen or fallen. Maintenance of such doubt would be successful stabilization and would coincide with avoiding any considerable disequilibrium.

One qualification should be made. Constancy in the value of money indicates continued equilibrium only if individual prices and wages are flexible enough so that disequilibrium *would* show itself in a price-level movement. If incipient price-level changes are to give signals for necessary adjustments in a tentatively chosen rate of money-supply growth, then individual prices and wages must be free. Ceilings and floors on individual prices and wages bring to mind Wilhelm Röpke's aphorism, "The more stabilization, the less stability."<sup>17</sup> Röpke's wise insight calls for overall stabilization measures rather than for myriad special interventions.

In short, it is more appropriate for the value of money to be stable than sticky.<sup>18</sup> Stickiness in the value of money is poor responsiveness to forces trying to change it; stability is steadiness through avoidance of forces trying to change it.<sup>19</sup>

This paper says nothing about *how* the quantity of money might best be regulated. Nothing said here necessarily provides a case for (or against) traditional monetary policy proper in preference to regulating the money supply through government budget surpluses and deficits. The cash-balance approach does, however, clarify the case for deliberately regulating the money supply somehow. An understanding of this case should help overcome superstitious qualms about creating money outright to pay for deliberate anti-depression open-market operations or government budget deficits.

<sup>17</sup> *Die Lehre von der Wirtschaft*, 4th ed. (Erlenbach-Zürich, 1946), p. 268.

<sup>18</sup> A. P. Lerner makes a suggestive distinction between stickiness and stability of an ordinary price. *Quarterly Journal of Economics*, May 1952, p. 186.

<sup>19</sup> George L. Bach has argued that stabilization of a flexible price level coincides with anti-depression and anti-inflation policy. "Monetary-Fiscal Policy, Debt Policy, and the Price Level," *American Economic Review*, May 1947, XXXVII, pp. 232, 236.

## MONETARY ASPECTS OF CHANGES IN TREASURY CASH BALANCES

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The monetary impact of Treasury operations in connection with what is commonly understood to be debt management—that is, sale, exchange, or redemption of debt instruments, determination of interest rates and maturities offered, and the related questions of debt ownership—has been extensively discussed in the economic literature of recent years. Such discussion has, however, largely neglected an aspect of the problem which we may refer to as asset management rather than debt management. This aspect, which came to the fore as an important part of the over-all Treasury function during World War II, is the size, distribution (as between commercial and Federal Reserve banks), and use of Treasury deposit balances. Since shifts and other changes in these cash balances may at times have considerable effect on credit conditions and monetary developments generally, an inquiry into the various factors involved may be fruitful.

From earliest times the Treasury made use of private banks in varying degrees as depositories for public monies. From 1863 to 1914, the depository function was shared by National banks and the subtreasuries. From 1914 to World War I, the Treasury's working balance was carried largely at the Federal Reserve banks, National banks being used as depositories only in special cases, such as those required by law in connection with postal savings accounts.

The use of private bank depositories for the carrying of funds derived from the sale of government securities was introduced during World War I, and was continued to a lesser degree throughout the interwar period. Until 1935 these Treasury accounts were exempt from reserve requirements, although until 1933 the depository banks were required to pay interest to the government on such balances. After 1935 (and until 1943) deposit accounts due the Treasury were subject to the same reserve requirements as other demand deposits. They remained small in size until World War II.

It was to be expected that Treasury balances would increase greatly during the war, particularly during and immediately after war bond drives. (Figure 1.) The use of War Loan Accounts, as they then came to be called, was greatly extended after 1943, when reserve requirements against these deposits were suspended. Although the exemption from reserve requirements was withdrawn as of June 30, 1947, banks have continued to maintain these accounts in the postwar period. Their use has been further expanded by the practice, begun in 1948, of permitting banks to credit to these accounts not only the proceeds of security sales, but also funds arising from payroll taxes under the old-age insurance and railroad retirement programs, certain excise taxes, withheld income taxes, and, in certain instances, large individual income tax payments which had formerly been deposited by the Director of Internal Revenue with Federal



Reserve Banks. In recognition of their broader character, these accounts are now known as Treasury Tax and Loan Accounts, and some eleven thousand banks are designated as depositories.

The Treasury does not draw checks against its numerous individual bank deposit accounts. Its working balance is kept at the Federal Reserve Banks, from which spending is implemented. As these Federal Reserve balances need replenishing, the Treasury issues "calls" to commercial bank depositories, requiring them to transfer funds to the Treasury account at Federal Reserve Banks. These calls vary in frequency with the size and location of the depository banks, and are normally in terms of a percentage of whatever amount the bank may be holding for the Treasury at the time of the call.

With a variable flow of funds into these accounts as securities are sold and taxes are collected, and with the changing needs of the Treasury for a working balance at Federal Reserve Banks for spending purposes, it is obvious that considerable variation in the size of the Treasury's total balances is possible; also, the proportion of the Treasury's total funds held at commercial banks may fluctuate substantially. Spending by the Treasury out of its Federal Reserve balances without a commensurate building up of those balances by calls on commercial bank depository accounts will increase member bank reserves; on the other hand, calls on depositories in excess of current spendings out of the Treasury's Federal Reserve working balance will reduce member bank reserves. The management of these funds thus gives the Treasury a means of altering the reserve position of member banks which is of no little importance.<sup>1</sup>

Federal Reserve authorities and member bankers have of course been aware of the effect of shifting Treasury balances on member bank reserves. However, judging from official statements, relatively little significance was attached to these shifts until fairly recently. In the course of his testimony before the Joint Committee on the Economic Report in 1947, Marriner Eccles, then chairman of the Board of Governors, spoke of the need of member banks to meet tax withdrawals arising from the budgetary surplus. He clearly assumed that tax receipts flowing into depository banks would automatically be called into the Reserve banks. Neither he nor any of the Treasury officials, private bankers, or economists who testified made any reference to the impact of Treasury balance shifts. Nor did the interested officials make any clear-cut statement of the relative effect upon the credit situation of the use of the Treasury surplus to redeem debt held

<sup>1</sup> For example: In the fiscal year 1953-54, Treasury balances at depository banks varied from a low of \$2.4 billion to a high of \$6.7 billion (month-end figures), averaging \$4.3 billion. Assuming that it might have been practical to transfer as much as half of the average amount (or about \$2.2 billion) from commercial banks to Federal Reserve Banks at any one time, it may be seen that such a transfer would have placed as much pressure on member banks reserves as an extremely large open market sale of government securities, or as an increase of two percentage points in the legal reserve requirement of member banks against demand deposits. The possible impact of a transfer in the opposite direction (from Federal Reserve Banks to commercial banks) would be much more limited, since balances held at Federal Reserve Banks generally have been much smaller (they averaged \$0.8 billion in 1953-54).

by the commercial banks as against its use to redeem Federal Reserve-held debt. Mr. Eccles spoke repeatedly of the use of this surplus to redeem "bank-held" debt, which would, he said, "reduce the money supply by an equivalent amount."<sup>2</sup> The term "equivalent amount" might be taken to mean that he assumed that the surplus would be used to redeem debt held by commercial banks; the deflationary effect would of course be much more pronounced if the funds were transferred to the Federal Reserve Banks and used to retire debt held by those institutions.

The whole question of policy concerning the use of Treasury balances arose immediately after the war, in connection with the large balances held by the Treasury at the end of the war. Early in 1946, at the time of the Victory war loan drive, total Treasury balances reached a high of \$26 billion, of which more than \$24 billion was held at commercial bank depositories. Once it became clear that the immediate postwar problem was inflation, not deflation, it was felt by all concerned that these large balances should be handled in such a way as to secure the maximum deflationary effect.

It is apparent that in the disposition of Treasury balances held at commercial banks three courses of action were possible: (1) They could be transferred to Federal Reserve Banks and there used to retire debt held by the latter. The result would of course be to put pressure on member bank reserves. (2) They could be used directly to retire debt held by the commercial banks. Up until mid-1947, when member banks were not required to hold reserves back of Treasury deposits, the credit impact of such a program would be negligible, since Treasury deposits and bank holdings of governments would be simultaneously reduced, with no direct effect on the reserve position of member banks. After the restoration of reserve requirements for Treasury deposits in 1947, the effect of the maneuver would be to reduce deposit liabilities of commercial banks without reducing their reserves, thus actually improving their reserve position and easing credit. (3) The balances could be held inactive in Treasury War Loan accounts, thus having no immediate effect on the reserve position of commercial banks, but assuring that that part of the money supply represented by Treasury deposits would not be used to feed the inflationary demand for goods. After mid-1947, with the re-imposition of reserve requirements, considerable pressure would have been put upon member bank reserves due to the sudden increase in deposits subject to reserve requirements.<sup>3</sup>

Undoubtedly Federal Reserve and Treasury staff and officials were aware of the main implications of the three alternative programs just mentioned. But there is considerable evidence that they were over-impressed with the deflationary effect of redemption of debt held by commercial banks, and that the third

<sup>2</sup> "Anti-inflation Program," hearings before the Joint Committee on the Economic Report, Eightieth Congress, 1st Session, p. 137.

<sup>3</sup> For an unusually clear statement of the various possibilities in connection with the retirement of public debt out of current receipts and out of Treasury balances see Lawrence F. Ritter, "A Note on the Retirement of Public Debt During Inflation," *The Journal of Finance*, March, 1951, pp. 66-70; see also Alvin H. Hansen, "How to Manage the National Debt—Comments," *The Review of Economics and Statistics*, February, 1949, pp. 30-31.

alternative, that of holding the balances idle, at least for a time, was given scant consideration.<sup>4</sup> Chairman Eccles, in testimony before the Joint Committee on the Economic Report in 1947, spoke of the use of Treasury funds to retire debt held by commercial banks as "reversing the inflationary process."<sup>5</sup> Later (1949) the new chairman of the Board of Governors, Thomas McCabe, said in a statement to the same committee that the dominant anti-inflationary factor of the postwar period was the use of Treasury balances to retire maturing securities, and that to the extent that such securities were held by commercial banks some decline in the volume of bank credit was brought about. He referred specifically in this connection to the balances held by the Treasury at the end of the war. Although technically debt redemption out of these balances reduced total deposits in the economy, it is difficult to see what deflationary effect resulted from the reduction of idle government deposit accounts, except the doubtful effect of a slight reduction in the over-all asset liquidity of banks. Secretary of the Treasury Snyder, in a statement to the Joint Committee in 1952, again referred to the deflationary effects of the use of the large postwar Treasury balances to retire public debt, "*particularly that held by the commercial banking system.*"<sup>6</sup>

Nowhere in these statements is there any mention of the possibility that greater deflationary impact might have been secured by holding Treasury deposits idle, rather than using them merely to soak up debt held by the banking system. The implication is that reduction of deposits, even Treasury deposits, is deflationary. Even when one makes allowance for the over-all limitations placed upon monetary policy by the necessity for bond price pegging, it would not appear that the Treasury balances on hand at the end of the war were most effectively used to curb inflation; and to the extent that circumstances forced the use of most of the funds for the redemption of debt held by commercial banks, it was hardly realistic to argue that such use was a deflationary force of great consequence.

As has been indicated, most of the discussion of the impact of Treasury operations upon monetary policy in the early postwar period centered around the use of the large balances remaining from the final war bond drive and of funds arising out of an excess of current receipts over expenditures. Although the handling of these funds by the Treasury was necessarily closely concerned with the relative size of Treasury balances at commercial banks and at Federal Reserve Banks, there is little evidence that this latter factor received much attention. As may be seen by referring to Figure 1, the radical variations in the size of Treasury deposit accounts which were a necessary concomitant of war finance were largely reflected in the Treasury's deposits at commercial banks rather than in its Federal Reserve Bank deposits. During the war, when Treasury balances were not subject to reserve requirements, this method of handling Treasury funds avoided placing any pressure on member bank reserves as a result of Treasury

<sup>4</sup> Treasury thinking at the time was undoubtedly influenced by a natural desire to reduce the total of debt outstanding as rapidly as possible.

<sup>5</sup> "Anti-inflation Program," *op. cit.*, p. 155.

<sup>6</sup> "Monetary Policy and the Management of the Public Debt," Eighty-second Congress, 2nd Session, pp. 60-61. (Italics supplied.)

finance operations during and immediately following bond drives. After the war, the bulk of the large Treasury balances held in commercial banks were used to retire debt held either by the public or by commercial banks, and little pressure on member bank reserves resulted.

Although the fact that shifts of Treasury funds between commercial bank depositories and Federal Reserve checking accounts have a direct effect on the general credit situation was undoubtedly well recognized by economists, Federal Reserve and Treasury staff members, and top officials of the two agencies, little explicit recognition of this fact is apparent during the postwar period. Professors Charles C. Abbott and G. L. Bach mention, in 1949 and 1950, the desirability of recognizing the importance of these shifts in the administration of monetary policy.<sup>7</sup> Passing comment on the subject appears in the 1947 annual report of the Board of Governors in the form of a statement that the total drain on bank reserves during the year was somewhat increased by a rise in Treasury balances at the Reserve Banks. There is no direct indication that this may have been part of a calculated program to put more pressure on member bank reserves.

Direct evidence of the use of Treasury deposit shifts to reinforce monetary policy may be found in a statement which appears in the July, 1947 issue of the *Federal Reserve Bulletin*: "In recent months . . . calls on war loan deposits have been timed so as to exert some temporary pressure upon the reserve position of commercial banks."<sup>8</sup> Further evidence that both Treasury and Federal Reserve officials had become more conscious of the importance of deposit shifts is found in a statement submitted by Chairman McCabe to a subcommittee of the Joint Committee on the Economic Report in 1949. Referring to the various situations in which Treasury and Federal Reserve officials consult with one another, he includes "the day-to-day variations in the Treasury's balance at the Federal Reserve banks and calls on balances with other depositories."<sup>9</sup>

In a statement made to another subcommittee of the Joint Committee on the Economic Report in 1952, Secretary of the Treasury Snyder describes in considerable detail Treasury policy with respect to deposit balances. He describes this policy as one of

minimizing the effects of seasonal or other fluctuations in cash receipts and disbursements on bank reserves, on Federal Reserve operations, and on the money market . . . made effective [by]: (1) handling the major share of the concentration in tax receipts through commercial banks rather than through Federal Reserve Banks, (2) permitting Treasury balances with the Federal Reserve to decline to a minimum immediately prior to tax collection peaks, and, on occasion, selling very short-term securities directly to the Federal Reserve System in anticipation of heavy tax receipts to cover what would otherwise amount

<sup>7</sup> Charles C. Abbott, "Notes on Federal Reserve Policy, August, 1945-June, 1948," *The Journal of Finance*, June, 1949, p. 102; G. L. Bach, *Federal Reserve Policy-Making* (New York: Knopf, 1950), pp. 145-146. For an earlier discussion see: Edward C. Simmons, "Treasury Deposits and Excess Reserves," *Journal of Political Economy*, June 1940, pp. 325-343.

<sup>8</sup> *Federal Reserve Bulletin*, July, 1947, p. 782.

<sup>9</sup> "Monetary, Credit, and Fiscal Policies," Statement submitted to the Joint Committee on the Economic Report, Eighty-first Congress, 1st Session, p. 28.

to a temporary overdraft in the Treasury's balance with the Federal Reserve Banks, and (3) the designing of public debt securities which will fall due on tax dates.<sup>10</sup>

Although Secretary Snyder did not indicate when the Treasury policy of minimizing seasonal shifts in reserve positions was adopted, it does not appear likely from a study of balances data that such a policy was pursued with any consistency prior to World War II. In both 1940 and 1941, for example, tax payment dates are marked by a sharp rise in Treasury balances at Federal Reserve Banks. Similar developments appear at several points during the war, although large Treasury operations in connection with bond drives obscure the picture. Following the war, and especially beginning in 1948, a more effective program of offsetting seasonal developments is apparent.

To what extent has the ability of the Treasury to influence the reserve position of commercial banks by shifting its balances been put to use as a monetary weapon in the control of cycles? Secretary Snyder, in his 1952 statement to the Joint Committee on the Economic Report, specifically referred to the use of this weapon in the 1948-1949 recession. He indicated that the Treasury built up its balances at Federal Reserve Banks "very considerably" in 1948 in order to assist in the restraint of inflationary pressures, and that as these pressures subsided in 1949 the balances were again allowed to run down.<sup>11</sup> This is clearly shown in Figure 1. During 1948 the average month-end balance at Reserve Banks was \$1.8 billion, almost exactly equal to the average of balances held at commercial banks; for the year 1949 the amounts were \$1.0 billion and \$2.3 billion respectively. One way of measuring the effect of this shift is to say that had the Treasury distributed its total balances between commercial and Reserve banks in the same proportion in 1949 as in 1948, member bank reserves in 1949 would have been lower than they were by more than \$.5 billion. (A similar comparison of 1948 with 1947 reinforces the general point that reserves were reduced during 1948 by shifting balances from commercial to Reserve banks.)

The distribution of Treasury balances during the rapid inflation that followed the outbreak of war in Korea in 1950 shows no evidence whatever of a policy of shifting balances to the Reserve Banks to restrain that inflation. Nor is there any clear indication of the use of this weapon in the 1953-1954 period. Although public statements made by both Treasury and Federal Reserve authorities have repeatedly stressed the extensive use of monetary weapons in the past two years, there is no hint that shifts in deposit balances were given a role. The data shown in Figure I do reveal some tendency since late in 1951 for the Treasury to hold a smaller proportion of its total balances at the Reserve Banks. Since mid-1953 total balances have declined somewhat, reflecting in part the necessary preoccupation of the Treasury with the problem of conforming with a restrictive limitation on total debt outstanding.

On balance, then, we cannot point to shifts in Treasury deposit balances as

<sup>10</sup> "Monetary Policy and the Management of the Public Debt," Subcommittee on General Credit Control and Debt Management, Joint Committee on the Economic Report, 82nd Congress, 2nd Session, p. 45.

<sup>11</sup> *Ibid.*, p. 46.



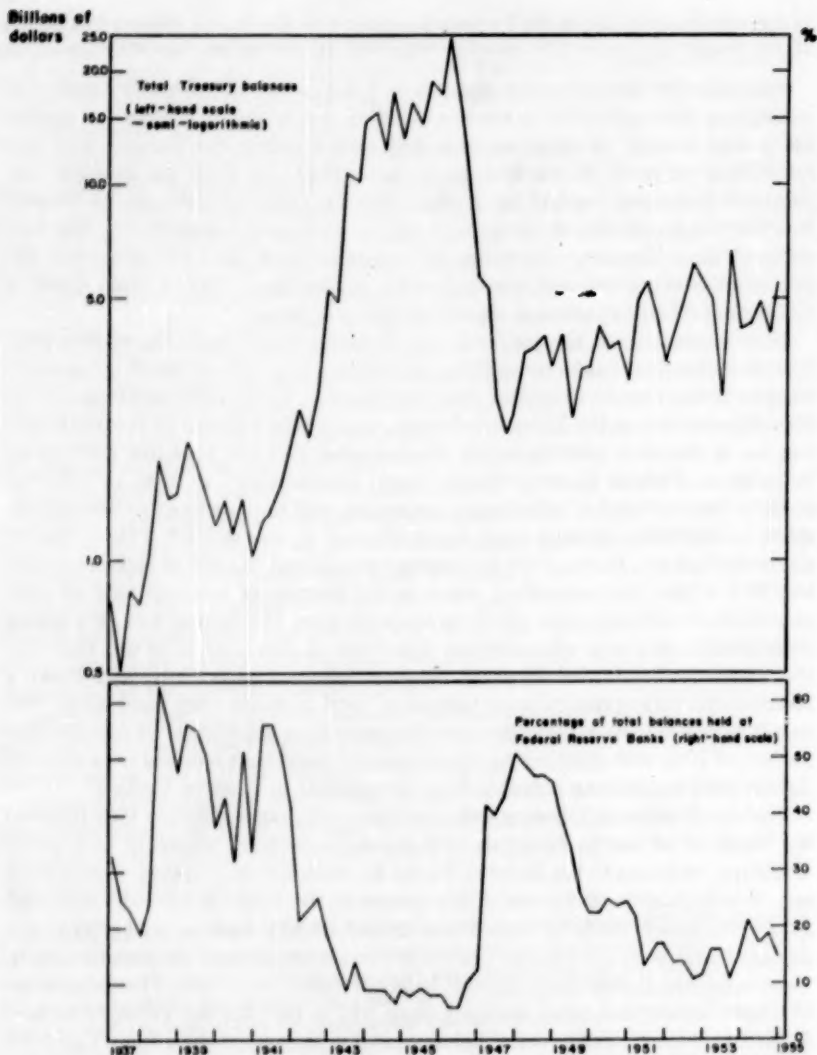


FIG. 1. TREASURY CASH BALANCES, 1937-1954  
(Quarterly averages of month-end figures)

Source of data: U. S. Treasury Department, *Treasury Bulletin*.

an important new monetary weapon. There is evidence of a growing realization in the past decade that such shifts have a significant effect on the reserve position of commercial banks and therefore on the credit situation generally. But, in spite of the conscious manipulation of balances in the 1948-1949 cycle, it is

clear that up to the present time only very tentative and limited use has been made of this weapon. In this connection, certain practical limitations must be recognized. Since the size of balances carried at the Reserve banks has for the most part been small, shifts from the central bank to private banks in large volume for the purpose of easing credit have not been possible. Really effective use of shifts in either direction would suggest that larger balances should be carried at both locations. The Treasury is reluctant to do this for a number of reasons, including the interest cost of additional debt to make such large balances possible, as well as the expressed doubts of some Congressional leaders concerning the necessity and desirability of carrying large balances at commercial bank depositories.<sup>12</sup>

During the postwar years the Treasury has quite naturally been preoccupied with its own problems of debt management. One result has been the much publicized conflict between the Treasury and the Federal Reserve System on the matter of higher interest rates during periods of inflation pressure. In spite of the apparent resolution of this conflict in favor of the Reserve System early in 1951, there is considerable reason to doubt that the Treasury has fully recognized its opportunities and responsibilities in the field of monetary control. Secretary Snyder, in reviewing in 1952 the anti-inflation program of the Government during the period after the outbreak of war in Korea, had this to say:

It seemed to the Treasury that an effective approach to the inflation problem required a broad program operating on many fronts. It required increased tax revenues. It required that the Government cut its expenditures in the nondefense area wherever practicable; and especially that the Government, as well as the public, exercise great restraint in the use of those goods and services which would be needed for our increased defense requirements. It required a strong program to promote greater savings. . . . This, indeed, has been the keynote of the Treasury's savings bond promotional efforts throughout the war and postwar decade . . . it was clear that the maintenance of sound economic and financial conditions during a period of heavy defense buildup required a program of other measures such as those asked for by the President and provided by the Congress in the Defense Production Act of 1950. Among these measures were selective controls.<sup>13</sup>

Here we have a considered statement of what the Treasury believed to be a complete inflation control program, and it makes no mention whatever of restraints on credit expansion or of the whole problem of interest rates or monetization of the debt! The fact is, however, that monetary policy which traditionally has been regarded as the province of the central bank, has become, by reason of the many fiscal developments that have occurred in the past fifteen years, a joint function of the Federal Reserve System and the Treasury. It is not a question of the monetary authorities asserting their independence of the Treasury. It is a question in the long run of a new approach by the Federal

<sup>12</sup> Note, for example, criticisms voiced by Representative Patman during hearings on the January 1954 Economic Report of the President (Hearings before the Joint Committee on the Economic Report, Eighty-third Congress, 2nd Session, pp. 69 ff.).

<sup>13</sup> *Op. cit.*, Eighty-second Congress, 2nd Session, pp. 68-69.

Reserve System and the Treasury—an approach which takes into account the new conditions and the new weapons. As E. A. Goldenweiser has put it:

... in the long run the solution of the problem of relationship between fiscal and monetary authorities will require fuller recognition by these authorities, by the Administration and by the public of the need of action in the monetary field with predominant reference to business conditions and without undue influence by the problem of debt-management.<sup>14</sup>

If the evidence to date shows that the Treasury stands in need of more "education" in this matter than does the Federal Reserve, this merely reflects the newness of the Treasury in the monetary field. The use of Treasury balances at commercial banks and at Federal Reserve Banks is only one of the monetary weapons which have been placed in the hands of the Treasury by the monetary-fiscal developments of recent times. The Treasury and the Federal Reserve need to evolve techniques of understanding and cooperation which will enable them to make the maximum use of this and other weapons for providing a greater degree of economic stability.

<sup>14</sup> E. A. Goldenweiser, "Douglas Committee Report," *American Economic Review*, June, 1950, p. 396.

## LABOR'S SHARE AND THE DEGREE OF UTILIZATION OF CAPACITY

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The purpose of this paper is to show the relationship between labor's share and the degree of utilization of capacity for the years 1929-1951. Several definitions of labor's share could be adopted, each of which would be preferable for an analysis of particular problems. Labor's share of either national income, private national income, or private non-agricultural income could be used. The latter two measures have the merit of excluding successively the not too appropriate governmental and agricultural sectors from the analysis. Labor's share of national income originating in government is, by arbitrary definition, 100 per cent. Labor's share of agricultural income cannot be measured by wages and salaries alone, because of the large number of small proprietors who work on their own farms and, hence, receive inseparable portions of both labor and non-labor incomes, in the form of proprietors' earnings.

Still, definitions excluding these sectors must be regarded as too restrictive. The increasing importance of government in both depression and war certainly influenced labor's share, as did the secular decline in agriculture. Therefore, labor's share is defined as wages and salaries expressed as a per cent of national income.

Although income of unincorporated enterprises is actually composed of both labor and non-labor shares, it is considered non-labor income in the National Income and Product Accounts. Thus, labor's share, by definition, increases as more economic activity is conducted through corporate establishments, even though labor's actual share may not have increased in a functional sense.

An "adjusted," deflated gross national product is employed as the basis for a derived measure of capacity. Adjustments are made to obtain an estimated value for deflated gross national product ( $GNP_d$ ) assuming that "full employment," with a "normal work week" obtained in each year during the entire period. Thus, it is necessary to compensate for the discrepancy between actual and "full employment" and between actual hours worked per week and the "normal work week." These adjustments implicitly assume a constant output per man-hour; however, the fitting of a trend to the resultant adjusted  $GNP_d$  figures largely overcomes this admittedly unrealistic assumption. In effect, the slope of the trend allows for the increase in labor productivity and the growth of capacity.

A third minor adjustment is necessary. The estimates of  $GNP_d$  include government outlays for work relief, while the data on employment do not include men on relief. To make the data comparable, deflated work relief is subtracted from  $GNP_d$ .

\*The writer wishes to acknowledge the helpful advice and criticism of Robert H. Johnson of the University of Iowa.

TABLE I  
The Derivation of "Normal Capacity" from Employment, Hourly, and Product  
Data, 1929-1951

Year	(1) GNP less work relief (Billions of 1939 dollars)	(2) "Full employment" Log trend of 97% labor force (Millions)	(3) Actual employment (Millions)	(4) "Normal" hours (Hours per week)	(5) Actual hours (Hours per week)	(6) Adjusted GNP (Billions of 1939 dollars)	(7) "Normal capacity" Log trend of adj. GNP (Billions of 1939 dollars)
1929	85.9	48.6	47.9	44	44.2	87.6	86.8
1930	78.1	49.0	45.7	44	42.1	87.8	89.2
1931	72.3	49.4	42.7	44	40.5	91.5	91.5
1932	60.7	49.8	39.2	44	38.3	89.6	94.0
1933	61.4	50.4	39.0	44	38.1	92.0	96.5
1934	66.4	50.8	41.1	44	34.6	103.8	99.1
1935	72.5	51.3	42.5	44	36.6	105.1	101.8
1936	81.6	51.8	44.7	44	39.2	107.4	104.5
1937	86.3	52.4	46.6	44	38.6	110.6	107.3
1938	81.9	53.0	44.6	44	35.6	120.4	110.4
1939	89.4	53.7	46.1	42	37.7	116.1	113.1
1940	98.4	54.3	48.0	46	38.1	117.1	116.2
1941	114.5	(55.0)	52.0	40	40.6	119.3	119.3
1942	129.3	(55.7)	57.7	40	42.9	116.5	122.5
1943	145.7	(56.5)	63.4	40	44.9	116.6	125.8
1944	156.9	(57.3)	65.3	40	45.2	120.9	129.2
1945	153.4	(58.1)	64.4	40	43.4	127.8	132.6
1946	138.4	59.0	59.0	40	40.4	137.0	136.2
1947	138.6	59.8	59.7	40	40.4	137.2	139.8
1948	143.5	60.9	60.9	40	40.1	143.5	143.6
1949	143.5	61.7	60.4	40	39.2	149.5	147.5
1950	154.3	62.8	61.4	40	40.5	155.8	151.4
1951	167.3	64.0	64.2	40	40.7	164.0	155.5

Col. (1): Deflated gross national product from ("National Income and Product of the United States, 1929-1950," table A, p. 146). Work relief (*ibid.*, table 14, p. 160). Deflator for work relief (*ibid.*, table B, p. 146). The price deflator for federal purchases of goods and services was used. Data for 1951 ("Survey of Current Business, National Income Number," July 1952, table 2, p. 13).

Col. (2): Labor force is composed of civilian labor force and armed forces. Labor force ("Handbook of Labor Statistics, 1950 Edition," table A-13, p. 35). The parentheses around the years, 1941-45, indicate that these years were excluded in arriving at the logarithmic trend. Data for 1951 ("Monthly Labor Review," February 1952, table A-1, p. 204). Estimated civilian labor force, 62.9 million, plus personal estimate of the armed forces 3.2 million (66.1 million).

Col. (3): Actual employment is composed of civilian and military employment. Civilian employment ("Handbook of Labor Statistics, 1950 Edition," table A-13, p. 35). Military employment (*ibid.*, table A-10, p. 29). Civilian employment for 1951 ("Monthly Labor Review," February 1952, table A-1, p. 204). The figure represents a monthly average of civilian employment. Military employment is estimated at 3.2 million.

Col. (4): "Normal hours" is derived from the provisions of the Fair Labor Standards Act.

Col. (5): Actual hours ("Handbook of Labor Statistics, 1950 Edition," table C-1, footnote 3, p. 59). Hours for all manufacturing are used. Datum for 1951 ("Monthly Labor Review," July 1952, table C-1, p. 95.).

$$\text{Col. (6)} = \frac{\text{Col. (1)}}{\frac{\text{Col. (3)} \times \text{Col. (5)}}{\text{Col. (2)} \times \text{Col. (4)}}$$

Col. (7): Fitted free-hand, using semi-log paper.



The adjustments described above are summarized in the following formula:

$$\text{Adjusted capacity } GNP_D = \frac{GNP_D - \text{work relief}_D}{\frac{\text{actual employment}}{\text{"full employment"}} \times \frac{\text{actual hours}}{\text{"normal hours"}}$$

These adjustments are explained in greater detail in order to make more explicit the meaning of the concepts, "full employment" and "normal hours."

"Full employment" in each year is defined as the trend value of 97 per cent of the labor force. A free-hand trend is fitted on semi-logarithmic paper to 97 per cent of the labor force for the years 1929-1951; the peak years of 1941-45 are excluded. Actual employment is defined as civilian employment plus personnel in the armed forces.<sup>1</sup>

The "normal work week" is defined in legal rather than in economic terms. The Fair Labor Standards Act of 1938 implied that the "normal work week" was to be considered 44 hours at that time; 42 hours in 1939; and 40 hours thereafter. In conformity with this, the "normal work week" is considered 44 hours from 1929 to 1938; 42 hours in 1939; and 40 hours for the remainder of the period. Actual hours worked per week are not available on an aggregative basis. As a substitute, hours worked in all manufacturing industries are used in the adjustments. This series may be more sensitive than total hours to changes in employment conditions.

The procedures employed in the formulation of the concept, "normal capacity," may be summarized as follows: Deflated gross national product was adjusted for the departure of actual from "full employment" and of actual work week from "normal work week." "Full employment" was defined as the logarithmic trend of 97 per cent of the labor force; "normal work week" was defined according to the provisions of the Fair Labor Standard Act of 1938. Deflated gross national product was also adjusted to exclude payments to persons on work relief. A logarithmic trend was then fitted free-hand to the adjusted  $GNP_D$  figures. This trend was labeled "normal capacity." These calculations are recorded in Table I.

#### RELATION BETWEEN LABOR'S SHARE AND THE DEGREE OF UTILIZATION OF CAPACITY

With definitions established the data may be examined for possible relationships between labor's share and the degree of utilization of capacity. First, deflated gross national product, less deflated work relief ( $GNP_D - WR_D$ ) is plotted against "normal capacity" in Chart 1, using the left-hand scale. In this same chart labor's share, ( $L/Y$ ), is plotted using the right-hand scale. By definition the economy is operating at "normal capacity" when the curve depicting  $GNP_D - WR_D$  intersects the "normal capacity" trend line. At these four points (approximately 1929, 1942, 1946-47, and 1950) lines are raised vertically to intersect the  $L/Y$  line. These points of intersection are marked with small circles. Reading from the right-hand scale, these points indicate the magnitude of

<sup>1</sup> The inclusion of members of the armed forces in the measure of employment is dictated by the fact that the military payrolls are a component of government purchases of goods and services and, hence of  $GNP$ .

labor's share at "normal capacity" rates of output. A free-hand trend line is then drawn so as to approximate the values of these four points in the years noted above. It is now possible to measure the discrepancy between labor's actual share and the "trend" of labor's share, and compare it with the departure of the economy from "normal capacity" levels of output.

From 1929-1941,  $GNP_D - WR_D$  is below "normal capacity"; from 1929-1940,  $L/Y$  is above its own "trend." Furthermore, these two series move almost in reciprocal fashion. Both show the greatest departure from their respective "trends" in the same years, 1932 and 1933; both reverse their general direction for the sharp recession of 1938. It appears, then, that labor's share increases as the degree of utilization of capacity decreases from "normal."

The next period is one of greater than "normal capacity" rates of output. From 1942-1946,  $GNP_D - WR_D$  is greater than "normal capacity"; from 1943 to 1947,  $L/Y$  is in excess of its "trend." The movement in both series is quite similar during this period. Thus, labor's share rises as the degree of utilization of capacity increases from "normal."

During the next period, 1946-1951,  $GNP_D - WR_D$  fluctuates near "normal capacity," while  $L/Y$  does not depart significantly from its "trend."

Thus, for the 23 year period, labor's share rises as the degree of utilization of capacity either increases or decreases from "normal." Labor's share is low and comparatively stable in the zone of "normal capacity."

Correlation analysis is used to establish an additional check on the above relationship, and also to measure the effect of changes in the degree of utilization of

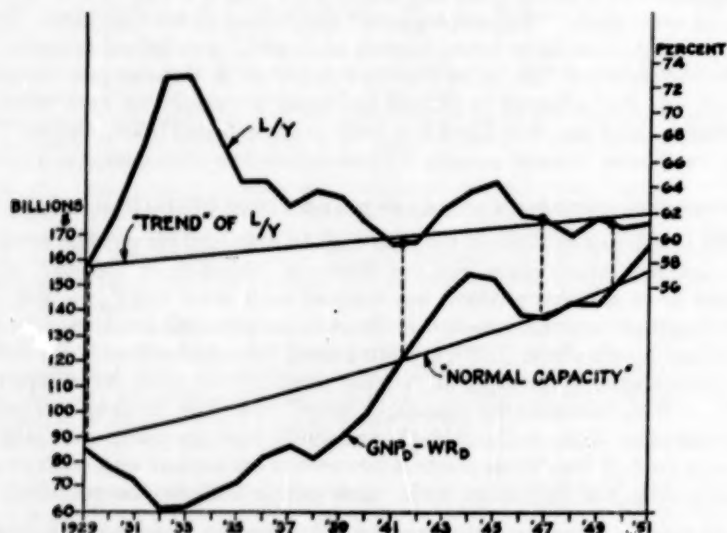


CHART 1

$GNP_D - WR_D$  AND "NORMAL CAPACITY" COMPARED WITH  $L/Y$  AND ITS "TREND"

TABLE II  
*Relationship of Labor's Share as a Per Cent of the "Trend" of Labor's Share to  
 the Degree of Utilization of Capacity, 1929-1951*

Year	(1) Degree of Utilization of Capacity X	(2) Labor's Share as a Per Cent of the "Trend" of Labor's Share Y
1929	99.0	100.0
1930	87.6	106.2
1931	79.0	114.2
1932	64.6	125.2
1933	63.6	125.1
1934	67.0	118.2
1935	71.2	109.7
1936	78.1	109.7
1937	80.4	105.8
1938	74.2	107.3
1939	79.0	106.2
1940	84.7	102.3
1941	96.0	99.3
1942	105.6	99.5
1943	115.8	103.5
1944	121.4	105.3
1945	115.7	106.3
1946	101.6	101.5
1947	99.1	100.6
1948	99.9	98.2
1949	97.3	100.3
1950	101.9	99.0
1951	107.6	99.0

Col. (1): Computed from "National Income and Product of the United States, 1929-1950" and Chart 1 of the article.

Col. (2): *Ibid.*

capacity upon changes in labor's share.<sup>2</sup> The degree of utilization of capacity is determined by dividing  $GNP_D - WR_D$  in each year by "normal capacity" in that year. Similarly, labor's share is divided by a "trend" of labor's share in each year. (See Table II) The analysis is separated into two periods, because  $L/Y$  rises both in periods of less than and greater than "normal capacity." Preliminary scatter diagrams were used to determine the point of division between periods; the first extends from 1929-40; the second, from 1941-51.

The results are:

$$1929-1940$$

$$Y_s = 162.1 - .70X$$

$$r = .86$$

$$F = 28.46; (.001 = 21.04)$$

$$1941-1951$$

$$Y_s = 73.6 + .26X$$

$$r = .81$$

$$F = 17.47; (.01 = 10.56)$$

<sup>2</sup> A somewhat similar test was employed by Louis H. Bean in his article, "Relation of Disposable Income and the Business Cycle to Expenditures," *The Review of Economics and Statistics*, 1947, Vol. 28, pp. 199-207.

Where:

$Y_e$  = Labor's share in a particular year expressed as a percentage of the "trend" of labor's share, or  $\frac{L/Y}{\text{"trend" of } L/Y}$

$X$  = Degree of utilization of "normal capacity" or  $\frac{GNP_d - WR_d}{\text{"normal capacity" } L/Y}$

Thus, for the period 1929-40, a decline of 20 per cent in the degree of utilization of capacity will yield an increase of 14 per cent in  $\frac{L/Y}{\text{"trend" of } L/Y}$ . A decline of 40 per cent in the degree of utilization of capacity will yield an increase of 28 per cent in  $\frac{L/Y}{\text{"trend" of } L/Y}$ . For the second period of an increase 20 per cent in the utilization of capacity will yield an increase of 5.2 per cent in  $\frac{L/Y}{\text{"trend" of } L/Y}$ .

One additional test is submitted as evidence of the existence of a relationship between the degree of utilization of capacity and the size of labor's share. Here, the data are arranged to show the difference of the means of labor's share at different percentages of "normal capacity" output.

First, the rate of utilization of capacity percentages are arrayed from low to high values (col. 1, Table III) with the corresponding values of  $L/Y$  (col. 2). Next, the figures for percentage utilization of capacity are grouped (col. 3) and the corresponding ratios of labor's share are averaged (col. 4). Finally, a measure of the significance of the differences of the means is ascertained and shown in col. 5. These results indicate that there is a significant difference in the means of grouped ratios of  $L/Y$  when the economy is operating at considerably less than or more than "normal capacity."

Labor's share increases as the degree of utilization of capacity increases from 100 per cent; it also increases as the utilization of capacity falls significantly below 100 per cent. Labor's share is both low and comparatively stable in the zone immediately around 100 per cent of "normal capacity" rates of production. The greater the departure from "normal capacity" output, the more significant is the difference of the means. The differences between groups I and II, II and III, and V and VI are highly significant. The differences between III and IV, and IV and V are not so significant; but this lack of significant differences is expected because the economy is operating near "normal capacity." So, both the significant differences between adjacent, extreme groups, and the lack of significant differences between adjacent, intermediate groups tends to establish confidence in the observed relationship between labor's share and the degree of utilization of capacity.

#### A SUB-AGGREGATIVE APPROACH

As the above capacity study is purely macroeconomic, it obscures some of the vital changes in the component parts of the aggregates. An examination of the changing composition of the economy at different percentages of capacity output

TABLE III  
Difference of the Means Test

Year	(1) Reverse Rank of Percentage of Capacity	(2) L/Y	(3) Percentage Capacity Grouped	(4) Arithmetic Mean of L/Y	(5) Frequency with which Difference of X's Would be this Large
1933	62.9	72.8	I. 60.0-69.9%	71.5	less than 1 time out of 1,000
1932	65.7	72.6			
1934	67.0	69.0			
1935	71.2	64.3	II. 70.0-79.9%	64.3	less than 1 time out of 100
1938	74.2	63.5			
1936	78.1	64.5			
1931	79.0	66.0			
1939	79.0	63.1			
1937	80.3	62.4	III. 80.0-89.9%	61.5	about 21 times out of 100
1940	84.7	61.0			
1930	87.6	61.2			
1941	96.0	59.4	IV. 90.0-99.9%	60.0	about 38 times out of 100
1949	97.3	61.6			
1929	99.0	57.4			
1947	99.1	61.4			
1948	99.9	60.1			
1946	101.6	61.7	V. 100.0-109.9%	60.9	less than 2 times out of 100
1950	101.9	61.0			
1942	105.6	59.7			
1951	106.0	61.2			
1945	115.7	64.4	VI. 110% and over	63.4	
1943	115.8	62.3			
1944	121.4	63.6			

Col. (5) The following formula is used to take into consideration the facts that  $N_1 \neq N_2$  and both  $N$ 's are small:

$$\sqrt{X_1 - X_2} = \sqrt{\frac{(N_1 + N_2)(\Sigma x_1^2 + \Sigma x_2^2)}{N_1 N_2 [(N_1 - 1) + (N_2 - 1)]}}$$

The 't' test was employed.

might provide an explanation for the relationship between labor's share and the degree of utilization of capacity.

Several years ago, Professor Dunlop analyzed the rise in labor's share during the depression in terms of the changing industrial composition of the economy.<sup>3</sup> He found that labor's share within an industry (the participation rate) increased most in those industries which decreased most in relative importance. This action prevented labor's share from rising even more during the depression.

The industries which decreased most in relative importance were the cyclically

<sup>3</sup> John T. Dunlop, *Wage Determination Under Trade Unions*, Macmillan Company, New York, 1944, Chapters VIII and IX.



sensitive industries,—extractive, heavy manufacturing, construction, etc.<sup>4</sup>

Dunlop employs short-run partial equilibrium analysis to explain changes in participation rates.<sup>5</sup> The theoretical model of the enterprise may be either a competitive firm, a monopolistically competitive firm, or a monopoly. The direction of change of the participation rate under conditions of declining demand will be the same in the above three types of firms, if the appropriate qualifications are made. However, as will be noted later, the magnitude of the change will vary with the elasticity of demand. "The inverse of the elasticity of demand is equal to the 'degree of monopoly power' when the enterprise is in short-run equilibrium."<sup>6</sup>

Paraphrasing Dunlop's explanation, the following initial assumptions will be made:

1. There are no changes in factor prices.
2. Labor is the only variable cost.
3. The firm has U-shaped marginal and average cost functions.
4. The firm is currently operating at the low point of the short-run average cost function.

Under conditions of competitive pricing a decrease in output resulting from a downward shift in the demand curve will yield an increase in the participation rate, ( $p_i$ ), because the demand curve will now intersect the marginal cost curve below the average cost curve. Thus, losses are incurred. The greater the decrease in the demand curve, the greater the loss, and hence, the greater the increase in  $p_i$ . The participation rate will also increase under monopolistically competitive or monopolistic pricing so long as the ratio of price to marginal cost, ( $P/MC$ ), remains constant. The increase in  $p_i$  will be exaggerated if  $P/MC$  is falling, i.e., if demand is becoming more elastic.

It has been observed for the depression period, that labor's share is related to the degree of utilization of capacity and to the changing industrial composition of the economy. If, as appears reasonable, the industrial composition of the economy is related to the degree of utilization of capacity, then, a more complete explanation of the changes in labor's share is provided. For the early thirties labor's share increases as the degree of utilization of capacity declines from "normal." During this decline, those industries which decrease most in relative importance experience the largest increase in participation rates.

In contrast to the behavior of the early thirties, labor's share increases while deflated national income *rises* in the early forties. The period, 1940-1944, was selected for consideration because of this concomitant rise in labor's share and deflated national income and because of the marked change in composition of output influenced by the war.

An extension of Dunlop's inter-industry analysis discloses behavior quite

<sup>4</sup> For a classification of the cyclical sensitivity of various industries, see Simon Kuznets, *National Income and Its Composition, 1919-1938*, Vol. I, National Bureau of Economic Research, New York, 1941, pp. 206-208.

<sup>5</sup> *Op. cit.*, pp. 183-187.

<sup>6</sup> *Ibid.*, p. 187, footnote 1.

different from that of the preceding period.<sup>7</sup> Those industrial divisions which became relatively more important as the economy expanded beyond "normal capacity rates of output" were also the industrial divisions in which the rates of participation were rising.

The rapid rate of growth of some of the industrial divisions reflects not only the phase of the business cycle, but also the influence of war. It is exceedingly difficult to separate the influences of war and prosperity, because the two have coexisted in so many business cycles. However, it is doubtful that the federal government would have expanded to the extent that it did in a non-war prosperity. If the federal government were excluded the increase in labor's share from 1940-1944 would evaporate. That is, labor's share of *private* national income decreases from 56.7 per cent in 1940 to 55.8 per cent in 1944 (a decrease of 0.9 percentage points); while labor's share of total national income increases from 61.0 per cent to 63.6 per cent (an increase of 2.6 percentage points). The difference is attributable to the large increase in the relative importance of government whose participation rate approximates 100 per cent. Income originating in government was 10.8 per cent of national income in 1940, 18.6 per cent in 1944.

The relative decline observed in wholesale and retail trade is partially attributable to war-time controls on the production of consumer goods, while contract construction was curtailed by the diversion of materials and manpower to the production of military goods.

As mentioned above, the period of the early forties constitutes a departure from the general inverse relation between changes in labor's share and changes in deflated national income. The increase in the participation rate of the expanding industries is also a departure from the behavior observed in the depression period. This unusual behavior of the early forties is influenced by both prosperity and war:

Those industries which expanded most, probably had to pay the greatest wage increases in order to attract laborers. That is, wages had to be raised not only to overcome immobility, but also to outbid other expanding industries for the services of labor. This combination of increased wages and employment in an industry would increase the wage bill. In the absence of price increases, it would also increase labor's share (wage bill divided by income originating in the industry). And it is quite likely that wage increases were larger than price increases during the period of war-time controls, because wage controls could be rather easily circumvented by reclassifying or "upgrading" labor.

It is also doubtful that businessmen adhered to the usual profit maximizing assumptions, even in the limited range of entrepreneurial decision left them in a world of price and wage controls. That is, they probably did not adjust output so as to maximize short-run profits within the framework of prices and wage

<sup>7</sup> Dunlop based his calculations upon an early thirty-three industry classification. The author's calculations are based upon the current sixty-nine industry classification of the Department of Commerce. With one exception, though, industrial divisions rather than individual industries were used. This procedure was indicated by the somewhat arbitrary classification of new war-created industries.

rates set for them by the government. Thus, if short-run marginal costs were rising, output may not have been restricted so as to equate the short-run marginal revenue and cost curves. Rather the producers may have increased output if they felt that they were subject to decreasing average costs in the long-run.<sup>8</sup> The increase in output in the face of rising marginal costs would have had the effect of raising labor's share in the industry, or the participation rate.

Then, too, the public's attitude toward war-time "profiteering" probably influenced businessmen in their price and output policies. Thus, even if businessmen were attentive to their short-run marginal costs, it is doubtful that they would restrict output. They, no doubt, felt that the exaction of the full measure of monopoly profits might have led to public clamor for nationalization or tighter control of certain basic industries.

It is also possible that unions may have been engaging in all-or-nothing bargaining (i.e., they may have been setting both wage rate and number of employees to be hired).<sup>9</sup> In a tight labor market it is rather easy to tempt the employer to take "all" rather than "none." In this way the union could have increased both dimensions of the wage bill, leaving the employer no marginal decision. In the absence of price increases, this would have had the effect of increasing labor's share.

Wage increases were no doubt stimulated by the excess profits tax. If the employer was subject to the 95 per cent excess profits tax rate, he could have granted laborers a one dollar raise at a net cost of only five cents to himself. It became the practice of many employers to pass their excess profits around in the form of wage increases and bonuses and thus reduce their tax bills. This had the dual advantage of building good will, and acquiring and keeping laborers in a very tight market. Thus, the excess profits tax made it possible for employers to grant wage increases out of proportion to productivity increases.

The renegotiation provision in many government contracts probably benefited labor. This provision was used in many cases where goods were being produced for the first time and for which there were no adequate cost data. When contracts were renegotiated, profits which the government could establish as "excessive" were reduced.

Other factors, such as overtime work and the hiring of what were formerly marginal workers, no doubt contributed to the cost burden and to labor's share.

In short, the increase in labor's share during the expansion period was probably influenced by both prosperity and war. The war-time phenomena probably influenced labor's share to such an extent as to throw serious doubt upon the stability of the relationships between labor's share and the degree of utilization of capacity. In other words, it is by no means established that a period of "super-

<sup>8</sup> For observations on entrepreneurial behavior, see John M. Keynes, "Relative Movements of Real Wages and Output," *The Economic Journal*, 1939, Vol. XLIX, pp. 34-51.

<sup>9</sup> See Martin Bronfenbrenner, "Wages in Excess of Marginal Revenue Product," *Southern Economic Journal*, January 1950, pp. 297, 198.

Source: Computed from "National Income and Product of the United States, 1929-1950" and Table I of this article.

full" employment would be characterized by an abnormally high ratio of  $L/Y$  in the absence of somewhat special circumstances prevailing during the war years.

#### SUMMARY

Labor's share rises as the degree of utilization either increases or decreases from "normal." As the economy declined from "normal capacity" rates of output, the industry groups which decreased most in relative importance showed the greatest increase in rate of participation. The net effect of the shifts was to minimize the increase in labor's share.

As the economy expanded beyond "normal capacity" rates of output, those industry groups which increased most in relative importance also showed the greatest increase in rate of participation. The net effect of these shifts was to magnify the increase in labor's share. In spite of these reinforcing shifts, labor's share still increased less in the period of war-time expansion than in the depression of the early thirties.

## ECONOMIC ACTIVITY ANALYSIS<sup>1</sup>

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The volume edited by Professor Oskar Morgenstern reflects the important metamorphosis that has been making its way slowly but steadily into economic science for the last two decades or so. It may very well serve the purpose of making a prospective graduate student form an idea of the range of the analytical weapons he would be expected to include in his scientific panoply, should he aspire to an undisputed title of economist by the time he enters the profession. By merely reading the table of contents of *Economic Activity Analysis*, he would realize that he would be expected, if not to possess the dexterity of mathematicians like Y. K. Wong, M. A. Woodbury, C. Bernstein, R. Bott, J. P. Mayberry, T. M. Whitin and K. Menger (the contributors of purely mathematical or highly mathematical papers included in the volume), at least to be able to understand their contributions and to use the tools forged by them. And, if he would thumb the middle part of the volume, he would discover that this implies familiarity with concepts such as "modular-absolute convergence," "non-commutative complex Banach algebra with unit-element," "modulus functions of real or complex valued matrices," or "the Cauchy-Hadamard condition"—to mention only a few of the terms that may strike a blank in the mind of the prospective graduate student. A trend exists in our science that has already divided economists into two categories: the economists and the *so-called* economists—or to use the *boutade* of Professor D. H. Robertson—into initiated economists and fellow-idiots. Economists have already reached the point where they cannot converse with each other as they used to do a few years ago. And the surprising fact is that this is not due entirely to the, let us call it, reluctance of the non-mathematical economist to learn the language of his mathematical *confrères*. It is due in part to the fact that the mathematical economists often fail to grasp fully the economic problem and, fascinated by the elegance of algebra and calculus, denude this problem of all its economically relevant aspects. The volume edited by Morgenstern offers some illustrations of this tendency.

The book under review is composed of three parts which follow a preface-type introduction. Morgenstern's claims that the various papers included in the volume "are closely related to each other and should be read together" is, as we shall see, only partly justified. But *Economic Activity Analysis* does not seem to suffer from this shortcoming more than similar "collections" that have become increasingly fashionable in econometric literature during the last few years. On the other hand, the number of economists who, like Morgenstern, can journey through the volume with uniform speed and unwavering interest is extremely limited; few would attempt therefore to read all the papers "together."

<sup>1</sup> This essay is a review article on the book, *Economic Activity Analysis*, edited by Oskar Morgenstern. (New York: John Wiley & Sons, Inc.; Chapman & Hall, Ltd., 1954. Pp. xviii, 554. \$6.75.)



Part I deals with the Economic Properties of the Input-Output System and contains two papers of special interest to both the user and the nonuser of the Leontief model. Despite its incontestable merits as an analytical tool and as a superior descriptive mapping of the national economy—far better than the old-fashioned series covering only production, employment and national income—the path-breaking contribution known as the Leontief model is hinged upon certain theoretical assumption and practical procedures that make its usefulness as a planning apparatus highly debatable. The paper of Judith Balderston and Thomson Whitin on "Aggregation in the Input-Output Model" throws interesting light upon one troublesome problem connected with the application of this model to planning. The authors point out that, apart from a few special cost structures that are unlikely to be found in the real world, there is no optimum method of aggregation (pp. 108-9). Various criteria of aggregation can be easily conceived, but they lead to different methods according to the purpose in mind: "a method of aggregation which makes available the desired information for one purpose may be totally useless for another purpose" (p. 92). The authors have consolidated an  $18 \times 18$  matrix of the United States economy in 1939 into three  $8 \times 8$  matrices, using a different aggregation in each case (pp. 115 ff). The predictive coefficients<sup>2</sup> of the  $18 \times 18$  matrix were then compared with those of the aggregated matrices, only to lead to the conclusion that "Leontief was correct in his supposition that different aggregations give rise to different solutions" (p. 109). Now, in this conclusion the word "different" must not be interpreted in its narrow sense of "nonidentical," but rather as "significantly different." A brief inspection of the predictive coefficients (pp. 126 ff) reveals that even the coefficients of the nonaggregated industries display errors as great as 2200 per cent (p. 127)! The authors discuss Leontief's statement that "by comparing these alternative short-cut answers [of all possible aggregations of a  $98 \times 98$  matrix into a  $48 \times 48$  matrix] with the known correct solution of our problem, on the one hand, and with each other, on the other, it is possible to measure the comparative 'goodness,' i.e., operational efficiency, of alternative aggregative classifications" (quoted on p. 88). No doubt such a comparison can be made. But since the  $98 \times 98$  matrix itself is the result of a very extensive aggregation of thousands of individual industries, no conclusion can be derived from this procedure which compares *error* with *error*. Conceivably, it may be possible that the  $48 \times 48$  matrix that would be considered the worst on the basis of the proposed criterion, might constitute the best substitute for the true, but unknown, matrix. In view of the tremendous variability and great number of industrial cost structures in the real economy, the hope of Balderston and Whitin "that some theoretical criteria can be developed on aggregation to take the place of this trial and error checking method" seems unjustified, if not contrary to their own analytical findings.

A second source of difficulty for the Leontief model as a planning instrument

<sup>2</sup> The predictive coefficients show the increase in the gross output of commodity *i* necessary for an increase of one unit in the quantity of the commodity *j* available for nonindustrial consumption. In mathematical language, the predictive coefficients are "the elements of the inverted input-output matrix."

is examined in a very neat paper, "The Treatment of Foreign and Domestic Trade and Transportation Charges in the Leontief Input-Output Table" by P. Modlin and G. Rosenbluth. The authors point out that the present state of our statistical information does not reveal the exact allocation of such items as trade margins, imports, and transportation charges. Nor are the practices concerning trade margins and transportation charges uniform even within the same "industry." Some arbitrary conventions have to be adopted in order to deal with these unknowns. They vary from the usual proportionality-distribution to the complete omission of trade margins. But none of these procedures are sound (pp. 135, 138). The conclusion based on theoretical as well as factual analysis is that "the difference in results due to the method of accounting for imports and trade margins cannot be regarded as negligible or insignificant in any reasonable sense of these terms" (p. 165). However, the implications of this conclusion reach deeper into the matter than Modlin and Rosenbluth indicate. If the various methods of allocation of input items lead to significantly different results, what about the effect of the errors inherent in the statistical data from which the input-output coefficients are derived? The argument that these errors, as well as aggregation, could not constitute a ground of serious concern, because the input-output matrix would possess some vaguely formulated property of "stability"—reflecting the stability of the real production system—is certainly arresting. However, one should first investigate the question whether the input-output matrix reflects a stability comparable to that of a rock lying flat on one of its faces or to that of a mechanism regulated by a number of very sensitive thermostats. For if the first interpretation is correct, the importance of errors and aggregation becomes secondary; but if the second interpretation is correct, the smallest departure from the true values would have incalculable consequences on the allocation of resources predicted by the input-output machine.

Probably the most vulnerable area of the Leontief model when applied to prediction at the level of the national economy is the assumption of constancy of the input-output coefficients. It is therefore surprising that the authors of the papers included in Part I have paid so little attention to this difficulty, although they were aware of the fact that "the Korean War and the attendant scare-buying upset the possibility of verifying the results of [the study made by the Bureau of Labor Statistics], the area of 'normalcy' being postponed still further" (p. 64). One would hardly dispute the fact that during the periods of 'normalcy' the predictions obtained on the basis of the linearity assumption are helpful, provided "there are permanent statistical staffs to keep the tables abreast of significant technological changes" (p. 58). This would be tantamount to predicting the direction of a *smooth* curve by continuous readjustments of linear extrapolation. Unfortunately under the strain of exceptional events, *smoothness* vanishes from economic changes. The question then arises whether the linearity assumption can take care of such changes too, so that we may rely upon the predictive coefficients of a linear model for economic planning in case of war, granting that these predictive coefficients would be kept abreast of the latest technological changes.

The editor's inclusion of the other two papers of Part I in the volume seems unfortunate. Balderston's "Models of General Economic Equilibrium" offers an illustration of how easy it is to lose sight of the economic problem by becoming excessively concerned with the mathematical varnish. What is particularly striking in her paper is the crippled view of Walrasian economics—which greatly contrasts with those of economists like Joseph Schumpeter or Wassily Leontief.<sup>3</sup> This shortcoming is aggravated by her failure to mention the economic weaknesses of other mathematical models that she holds in high esteem merely because with them one can prove the existence of an equilibrium solution. She seems to ignore the reason why in the Walrasian system there are no fixed costs and states that this is an assumption introduced "for simplicity" (p. 6). It is also surprising to read that "a second criticism of the Walrasian system has been that free goods were omitted" (p. 16). Her appraisal of the Schlesinger-Wald model as superior to that of Walras overlooks the fact that the former does not exclude the possibility of a zero equilibrium wage-rate and that it assumes that in a nonmonetary system prices are absolutely determined. It is too easily forgotten that models, such as Schlesinger's or von Neumann's, were inspired primarily by mathematical considerations. This is particularly true of von Neumann's model, for which not even a far-fetched relation with a real economy has been so far forthcoming. And indeed it would be difficult to give any meaningful interpretation to the rate of interest as defined in von Neumann's model, in which labor is part of both cost and revenue.<sup>4</sup> One cannot possibly make any sense out of the assertion that this model "goes far beyond Walrasian economics" (p. 37). But Balderston's paper contains other statements, too numerous to be listed here, that would startle an economist. She attributes the main theorem of the Marginal Productivity Theory to Hicks and Samuelson (p. 20). Her derivation of the production function on p. 17 is obviously fallacious. Since her article is intended mainly as a critical appraisal of various equilibrium theories, one can but deplore the fact that in mentioning Cassel's approach (p. 7n), she fails to point out its main drawback: in contrast with the Lausanne system, Cassel's model does not provide a simple expression of the relationship that binds all possible demand curves *via* income. In the list of the assumptions underlying the Leontief model, she intermingles factual and logical considerations in a manner that is likely to confuse the reader (pp. 31-33). Conditions 2 and 8 are not independent and should not have been listed separately. The statement that in the Leontief model "prices are taken as given by the market," is incorrect: according to the rationale of Leontief's system prices are *determined* by the input-output coefficients. The second paper, "The Input-Output System—Its Nature and Use" by O. Eckstein, is mainly a summary of Leontief's own works. Its greatest short-

<sup>3</sup> See for instance Leontief's splendid review of Walras' *Éléments d'Économie Pure* in the October 1955 issue of this *Journal*.

<sup>4</sup> The difficulty is easily illustrated by an economy where the only process available is one in which "food" and "labor" increase by the same percentage, say, ten per cent, each year. The von Neumann interest rate is in this case ten per cent, but it is impossible to see who would earn it.

coming is that it ignores many of the criticisms of earlier writers regarding the use of Leontief's model for the analysis of such nonlinear economic phenomena as foreign trade or value added. Because of this, it can hardly be considered a positive asset to the volume.

Part II contains seven papers dealing with the Mathematical Properties of Linear Economic Systems. I will leave the task of appraising these contributions to the reviewers in mathematical periodicals. I wish, however, to point out that I found them very instructive. The orderly and comprehensive presentation of the properties of the matrices and trees used in the linear models constitutes one of the principal merits of the volume, both as a compact source of reference and as an advanced reading for the would-be econometrician.

Part III, somewhat unhappily entitled "Meta-Economics," consists of two papers having nothing in common and very little direct bearing upon the other contributions. In "The Laws of Return, A Study in Meta-Economics," K. Menger uses a formidable mathematical arsenal to bring home two main points: (1) diminishing marginal returns and diminishing average returns are not equivalent conditions, and (2) boundedness of the return function implies neither decreasing marginal returns nor decreasing average returns. Menger believes that many famous economists have committed numerous blunders by ignoring the above points, so he sets out to teach the economists a lesson in logical consistency. But the route he chooses does not lead to this goal. The argument is too tortuous even for a mathematical economist; Menger himself must have been aware of this since he supplemented the paper with a diagram—equally tortuous—of the various points proved in the text (p. 482). Thus, the ordinary economist—Menger's intended pupil—is barred by its language from following the lesson. Actually, the difference between the marginal returns and average returns is today emphasized by most textbooks—witness the rubber-stamp diagram of cost curves used in the theory of the firm. Menger's second point, that boundedness of the return function does not imply either decreasing marginal returns or decreasing average returns, may indeed be a novelty for the nonmathematical and even the ordinary mathematical economist—but certainly not for the better trained mathematical economist who from his theory of functions knows the current use of wiggling curves in proving all kinds of fallacies. Probably everybody would have been better off if Menger would have used only simple diagrams, such as those of pp. 427, 446, to prove the main points of his paper.

Frequent passages in Menger's paper (pp. 420–22, 443, *passim*) seem to have no other purpose than to show how silly even famous economists were. Probably, it would be possible to indict many such economists for inaccurate formulation of the two laws of returns and some for not being more explicit about the assumptions that justified the equivalence of diminishing marginal and diminishing average returns. But Menger's procedure of interpreting isolated passages independently of the whole conceptual framework is clearly unsound. By the same token, one may chide a mathematician for the statement that the angles of an equilateral triangle are equal to  $60^\circ$  on the grounds that he did not specify in the same sentence that he was dealing with a Euclidian structure. Menger ignores

the fact that economists assume the return function to have a *regular* shape—something like the Eulerian continua—and that a wiggling function such as that of p. 446, would be regarded as a definite symptom of the indivisibility of some factor of production. Before considering the law of diminishing returns, the economists denounced by Menger argued that the division of labor and capital caused the marginal return of a dose applied to an acre of land to increase, at least for the first doses. Only after this point was established did the economists raise the question whether the gains derived from the division of labor could continue indefinitely. Then, they noted that if the marginal return of a dose would continue to increase for *all* doses, there would be no reason whatever to use more than one acre of land: with sufficient number of doses, the output of the first acre can be brought to any level, while transference of the last dose from the first to the second acre would decrease the output. And as in the real world not all the wheat is produced on one acre, but on millions of acres, a simple *reductio ad absurdum* proves that the marginal return of a dose cannot increase indefinitely. But this is not all. The investment per acre must be the same for all acres, in fact it must be at least equal to the optimum amount of doses. This optimum corresponds to the highest average return. Consequently, in order to have more than one acre used, the average return also must decrease sooner or later. The law of returns must therefore possess some scale such that an additional dose would decrease the average return and would yield a lower return than the previous dose. This is the Law of Diminishing Returns—*tout court*. Economics does not need more than this to explain allocation of resources and scarcity rents. True, it is at times argued—not without some justification—that after the division of labor has reached the point where its gains are outbalanced by difficulties engendered by size, no other economic force can be thought of to repeat the feat of the division of labor; consequently, the return function of the firm has only one inflection point. But this is a secondary argument. Menger was able to reach the conclusion that the Law of Diminishing Returns has not been proved only because he decided to ignore the complete argument and the true interpretation of this law. A greater familiarity with economic theory would have led him to distinguish between the production function of the firm and that for all firms; the failure to do so is at the root of the whole misunderstanding.

But Menger aims to teach the economists a lesson in economics, too. He argues in all earnestness that it is advisable to use *only* the less fertile land if the average labor cost on it is smaller than that obtained by using all land of the first grade. He even supports this view by offering an illustration from the real world: the production of wild herbs and berries (pp. 456–57). Instructors of economics and members of doctoral examination committees will be glad to be provided with a new “smart” question: What is wrong with Menger’s argument that if the first-grade land is scarce, then only second-grade land may earn rent? Clearly, careful reasoning constitutes a universal desideratum, equally appropriate for all branches of human thought, not merely for the “fellow-idiot.”

Morgenstern’s paper is the only one in the volume that can, and should, be read by all economists independently of their interest in the input-output tech-



niques, for it offers an account of the impact of the invention of giant brains upon economic science by one of the economists best qualified to speak on this matter. Morgenstern's thesis is that the possibility of performing large-scale computations has produced a revolution in economics, and he supports this view by various pertinent examples. The analogy with meteorology, where the speed with which the weather travels competes with the speed with which the computations required for a more accurate prediction can be performed, is certainly impressive (p. 488). So is the description of the methods used by the Harvard Economic Project, the Princeton Institute for Advanced Studies, and the National Bureau of Standards, for dealing with large scale input-output matrices (p. 496). One can but agree with Morgenstern in predicting that the trend already observed in the use of linear programming techniques by business firms and governmental agencies will become accentuated in the near future. But this reviewer feels that Morgenstern attributes to the electronic computers a power that they do not possess; indeed they cannot solve the *economic problem* unless we circumscribe this problem to *production under static conditions*. In fact, this is the way in which Morgenstern seems to regard the economic problem in the argument presented in his paper. One cannot deny that recent years have witnessed more than a revolution in economics. They have given birth to a new science without the death of the old. The new discipline which is still searching for an appropriate name wears various labels, such as linear programming, economic activity analysis, management science, etc. Probably "engineering economics" would be a more adequate title. Clearly, engineering economics would not have come of age without the large-scale computers.

Few economists and still fewer businessmen have as yet realized the tremendous usefulness of engineering economics. However, it would be a mistake to regard the impact of large-scale computation upon business management as essentially different from the impact of other technological innovations affecting the size and nature of the information on which businessmen base their decisions. The telegraph, the telephone, the IBM machines, etc., have had a similar influence upon entrepreneurial decision making: they increased the evidence available at the moment of decision. A still greater error would be to hold that the electronic computers have helped economic science advance more than did the IBM computers. Progress in the theories of utility, uncertainty and technological change of the last ten years came from entirely different directions, the same that inspired the Lausanne School, the famous Harvard Economic Barometer of W. Persons, and the works of H. Schultz on the statistical laws of demand and supply, for instance.

Morgenstern seems to think also that the electronic computers "may reveal decisive properties of demand, money, preference, etc. about which we suspect nothing at present" (p. 517n). He even thinks that they have opened the door for true experiments in economics of the types used in the natural sciences, and that the only limitation may come from the exorbitant cost of such experiments in the social sciences (p. 512). I do not believe that many economists would go along with Morgenstern on this matter. In the first place, economics as well as

the other social sciences have to put up with a difficulty that is almost unknown—or is for all practical purposes irrelevant—in natural sciences: this is the problem of the Nonconstancy of Economic Laws and the decisive disturbance of any experiment upon the observed economic unit. What use can be made of the results of an experiment when we know that the unit experimented with is changed by each new experimental condition and that it is almost impossible to find two economic units absolutely identical? In the second place, economics has also to contend with evolutionary or historical changes, which do not always repeat themselves. An experience of this reviewer is almost tailored to illustrate this point. In 1947, a fabulous monetary inflation occurred in Rumania, unique in that the increase in prices lagged well behind the increase of currency by the central bank. The explanation of this result—quite surprising for monetary theorists—lies in the fact that the Rumanian peasant was fascinated by the idea of becoming a multi-millionaire and hoarded most of the banknotes in circulation. What experiments could one have suggested to predict the monetary paradox of 1947 in Rumania, save the one that led to it? And now that we have on record this experience, what can be concluded regarding another monetary inflation in Rumania? No matter how perplexing may be the fundamental problems of economics, ignoring them would serve no purpose.

A final word of caution to the reader is in order about the inexcusably large number of printing errors and of missing page numbers in cross-references. They may overtax the patience of a reader not entirely familiar with all the aspects of the subjects treated in the volume, while all readers are likely to deplore the absence of a subject-index particularly necessary in a volume of this kind. The prestige of activity analysis economists is not likely to be enhanced by the little attention the authors of the volume paid to the elementary conditions of success in their own economic activity.

## COMMUNICATIONS

### PROFESSOR MACHLUP ON VERIFICATION IN ECONOMICS

According to Professor Machlup it is significant to distinguish two schools of thought on the subject of verification in economics, which he describes as the "A Priori" and the "Ultra-Empiricist." Of the "Ultra-Empiricist" he writes: "This again is the essence of the ultra-empiricist position on verification: the ultra-empiricist is so distrustful of deductive systems of thought that he is not satisfied with the indirect verification of hypotheses, that is, with tests showing that the results deduced (from these hypotheses and certain factual assumptions) are in approximate correspondence with reliable observational data; instead, he insists on the independent verification of all the assumptions, hypothetical as well as factual, perhaps even of each intermediate step in the analysis." (Italics added.)<sup>1</sup> In fact Ultra-empiricists "refuse to recognise the legitimacy of employing at any level of analysis propositions not independently verifiable" (p. 7).

Professor Machlup claims that he could give "dozens" of examples of the "Ultra-Empiricist" position. The one he chooses to cite is that outlined in my book *The Significance and Basic Postulates of Economic Theory* (1938). He makes it clear that he is not concerned so much with extracting a single statement or two which is at fault, but with the position represented throughout the book—"I have selected Hutchison" Professor Machlup writes).

I find I wrote (*op. cit.*, p. 9): "If the finished propositions of a science, as against the accessory purely logical or mathematical propositions used in many sciences, including Economics, are to have any empirical content, as the finished propositions of all sciences except of Logic and Mathematics obviously must have, (6) then these propositions must *conceivably* be capable of empirical testing or be reducible to such propositions by logical or mathematical deduction. They need not, that is, actually be tested or even be *practically* capable of testing under present or future technical conditions or conditions of statistical investigation, nor is there any sense in talking of some kind of "absolute" test which will "finally" decide whether a proposition is "absolutely" true or false. But it must be possible to indicate intersubjectively what is the case if they are true or false; their truth or falsity, that is, must make some conceivable empirically noticeable difference, or some such difference must be directly deducible therefrom." (Italics as in original.) The note (6) attached to this passage ran: "This seems to us obvious. But the contrary view that Economics is, or ought to be, not an empirical science at all but a formal science just like Mathematics and Logic is (1937) held by a number of authorities led by Professor L. von Mises. Cf. *Grundprobleme der Nationalökonomie* and his lecture in *Actes du Congrès Internationale de Philosophie*, Paris, 1937. In future references we may, for reasons of brevity, omit this obvious qualification to the Principle of Testability: that a scientific proposition may not itself be empirically testable *directly*, but may be reducible by direct deduction

<sup>1</sup> Cf. F. Machlup, "The Problem of Verification in Economics," *Southern Economic Journal*, July 1955, p. 8.

to an empirically testable proposition or propositions (cf. propositions of Physics about electrons,  $\alpha$  and  $\beta$  particles, etc.)."

This was the first and only relatively full account of the position on verification which I tried to expound in my book. This passage now seems to me rather old-fashioned, and even slightly crude and ungrammatical in the way it is formulated. But one thing it indubitably is *not*, and that is an example of what Professor Machlup calls "Ultra-empiricism." In fact it explicitly denies what he describes as "the essence of the ultra-empiricist position on verification."

Fortunately I do not have to rely on my own interpretation of my writings of eighteen years ago, if any interpreting is necessary. In his work *Economic Theory and Method* (recently published in a new English edition), Professor F. Zeuthen makes it clear that he is quoting me in diametrically the opposite sense to Professor Machlup, (and I venture to assume that Professor Zeuthen would not have chosen to quote me in that sense, or any other, if there had seemed to him to be any question of the direction my argument was taking). Professor Zeuthen writes: "If statements about reality are to have a meaning, and if they are not direct statements as to individual observations, it must be possible, by means of logical transformations to translate them at least into possible observations. There must be a possibility of verifying their reality or the reality of their consequences. In a rationalized theory, as, for instance, in micro-physics, it is still not considered necessary to be able to translate each individual statement into the language of reality, if only verification of a certain complex of statements is possible. In this connection we may also quote Paul Samuelson: 'By a meaningful theorem I mean simply a hypothesis about empirical data which could conceivably be refuted, if only under ideal conditions.' ... Direct or indirect measurability (or the possibility of other factual testing) is a necessary condition for the avoidance of mystery, where everyone may have his own ideas as to the same words. Scientific statements about reality must be verifiable by others. As Hutchison says, they must 'conceivably be capable of empirical testing or be reducible to such propositions by logical or mathematical deductions. If there is no conceivable possibility of proving if an assertion is right, it is of a mystical character' " (*op. cit.*, pp. 8-9).

I am afraid it seems to me that—doubtless through my own fault—Professor Machlup completely failed to understand the position I was trying to outline, particularly since not a single one of the very brief passages of mine which he quoted seems to me, when taken in context, to make the point which Professor Machlup seems to imagine it makes.<sup>2</sup> Professor Zeuthen may be easier to follow,

<sup>2</sup> I don't wish to claim any particular wisdom or rectitude for my propositions, only that Professor Machlup has not interpreted accurately what he very briefly quotes. For example (a) Professor Machlup quotes me as writing "that propositions of pure theory, by themselves, have no prognostic value," and states that this proposition, "as it stands," is "unassailable." However, in his determination to assail the unassailable, Professor Machlup proceeds to interpret "propositions of pure theory, by themselves, have no prognostic value" as meaning "an attack against the use of empirically unverifiable propositions in economic theory regardless of their conjunction with other propositions." (b) Professor Machlup writes, "With regard to the 'fundamental assumption' of economic theory con-

and it might help to elucidate Professor Machlup's categories if he could explain whether Professor Zeuthen fell into the category of "Ultra-Empiricists," or that of "A Priorists"; or indeed how the other important contributors in the last decade, to the methodology of economics, such as Samuelson, Lange, Little and Friedman are to be placed in relation to these categories.<sup>3</sup>

While the trouble with Professor Machlup's "Ultra-Empiricist" category simply seems to be that the one example he gives falls quite obviously outside it, the trouble with his "A-Priorist" category seems to be that it is much too elastic and comprehensive to be significant, while at least one or two of the various authorities Professor Machlup describes as "A-Priorists" might well have much preferred to be called "empiricists," if they were to be called anything. Professor Machlup agrees that his term covers writers of very different epistemological views, ranging from J. S. Mill to Mises. After telling us (p. 5) that he is simply concerned with two "extreme positions" Professor Machlup proceeds, while indeed defining "Ultra-Empiricism" in extreme terms, to leave "A-Priorism" very elastic. In fact it is very hard to tell whether his two categories are meant to describe two extremes, with a large third middle ground in between; or whether "A-Priorism" is being so stretched as to include all the middle ground up to the frontier line of "Ultra-Empiricism," the former comprising all those who are prepared to recognize "indirect" methods of verification or confirmation, and the latter those who explicitly reject indirect verification and insist on "direct" in-

cerning 'subjectively rational' and maximising behaviour, Hutchison states that 'the empirical content of the assumption and all the conclusions will be the same—that is nothing.' Here I would simply like to quote my complete sentence which was concerned with Professor Mises' apparently circular method of formulating the fundamental assumption (not with other methods): 'If one thinks it worth while, one can say 'people behave as they do behave' in as many different ways as one likes, but one will not learn anything further about their behaviour; for the empirical content of the assumption and all the conclusions will be the same—that is nothing.'

<sup>3</sup> Cf. the following passage from Prof. Zeuthen's chapter on Material and Method in *Economics*, *op. cit.*, pp. 14-15: "How the conception of economics as an empirical, i.e., a logical-empirical science is compatible with a considerable amount of deduction and theorizing will be apparent from the following statement by O. Lange: 'Theoretical economics puts the pattern of uniformity in a coherent system. This is done by presenting the laws of economics as a deductive set of propositions derived by the rules of logic (and of mathematics) from a few basic propositions. The basic propositions are called assumptions or postulates, the derived propositions are called theorems. Theoretical economics thus appears (like all other theoretical sciences) as a deductive science. This, however, does not make it a branch of pure mathematics or logic. Like the rest of economics, economic theory is an empirical science. *Its assumptions or postulates are approximative generalizations of empirical observations; e.g., the assumption that business enterprises act so as to maximise their money profit.* Some inaccuracy of approximation (e.g., some considerations, like safety, may keep enterprises from maximizing money profit) is accepted for the sake of greater simplicity. The theorems, in turn, are subjected to test by empirical observation. A deductive set of theorems to be subjected to empirical test is also called a theory, hypothesis, or a model. We can thus say that theoretical economics provides hypotheses or models based on generalizations of observations and subject to empirical test. Since the assumptions (postulates) underlying a model are only approximative, the theorems do not correspond directly to results of empirical observations.' " (Italics added.)



dependent verification or confirmation only (assuming Professor Machlup can give an example of this category).<sup>4</sup>

However, it seems doubtful whether any distinction which is made to turn on whether or not "indirect" verification or testing is accepted, could be at all serviceable—even if it were more lucidly defined, less questionably labelled, and less erroneously exemplified. Supposing (A) I have tested and confirmed (1) that a plot of ground forms a right-angled triangle, and (2) that the two shorter sides are 30 and 40 yards long; and supposing (B) that I have checked my calculation or deduction via the Pythagoras theorem that the longest side is 50 yards long. Professor Machlup apparently insists that there are "dozens" of economists who would *deny* that the performance of these measurements and tests, as to the two shorter sides and the right angle, entitled me to regard 'as to that extent tested and verified the proposition (C) that the third side was 50 yards long? These "dozens" of "Ultra-Empiricist" economists (whose existence I beg leave to doubt) would continue to regard proposition (C) as a completely unconfirmed piece of speculative guess-work until I had tested or confirmed it "directly" and "independently" by separately measuring the 50-yard side (which might conceivably for technical reasons be very difficult or practically impossible).

Anyhow, it would not seem to be committing some incredibly naive and dangerous methodological error if I *did* attempt to test (C) directly and independently by a separate measurement, provided this was technically or practically possible. Whether (C) was tested directly or indirectly would be a matter of practical convenience and of the degree of confirmation aimed at. It is not clear how any serious controversial point can arise here, or how in such a case any very interesting distinction can be made to turn on whether or not the "indirect" testing of (C) is acceptable or not.

So much for the critical-historical elements in Professor Machlup's paper. Perhaps, now I have started, I may go on to express one or two doubts about his more positive thesis. The point at issue,—and there is a point at issue,—lies rather in Professor Machlup's conception of "fundamental assumptions" or "high-level generalizations" in *economics*. The only example he gives of this special type of proposition is "the fundamental assumption" that "people act rationally, try to make the most of their opportunities, and are able to arrange their preferences in a consistent order; that entrepreneurs prefer more profit with equal risk" (pp. 10–11). These are all variations on the ubiquitous assump-

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<sup>4</sup> One function of this elastic category "A Priorist," which is first described as "extreme" but which is then stretched to include J. S. Mill, seems to be to cast an aura of respectable moderation on the certainly highly "extreme" political and methodological dogmatizing of Professor L. Mises. There have been previous examples in the last decade or so of associates or disciples of Professor Mises volunteering such explanations as that when Mises said "impossible" he really meant "possible," or when he said "a priori" he really meant "empirical." Now, according to Professor Machlup, when Professor Mises held that in economics "the fundamental postulates are *a priori* truths, necessities of thinking" (p. 6) "all" he "had in mind however provocative (his) contentions seemed" as an "objection . . . to verifying the basic assumptions in isolation."

tion, the central assumption in 'micro-economic' analysis, of 'maximising' or 'rational' action. It might be helpful to know whether Professor Machlup can cite any other examples of a "fundamental assumption" in economics beyond this one and its variants. If so, the point at issue might well be illuminated, while if not, it would stand out in a pretty clearly defined way as turning on the status and nature of this proposition about "maximising" and/or "rational" conduct.<sup>4</sup> We should like to note here that Professor Machlup describes this "fundamental assumption" as "empirically meaningful," which would appear to mean "conceivably falsifiable empirically"; or, at any rate, Professor Machlup does not interpret this fundamental assumption as a more or less disguised definition, without empirical content, that is, as simply saying that people maximise what they maximise, or that economic conduct must, by definition, be rational—(as Professor Mises appears to hold).

Now the main difficulty with this fundamental assumption, throughout its history,—since, roughly speaking, Bentham,—has been that of knowing just what content, if any, it has been meant to possess, just when, where, and how far it is applicable, and therefore just what the significance may be of the conclusions about human activities which can logically be deduced from it. At one time this fundamental assumption was formulated to the effect that the consumer "maximised his satisfaction" or "utility," the firm its "profits," or even that society, in certain conditions, maximised its aggregate "social satisfaction" or "utility" or "welfare." What was necessary, in the first instance, with such formulations of this fundamental assumption, was more *clarity* rather than more confirmation or verification, that is, not any actual testing so much as a specification of what a test would amount to, or of the more precise circumstances under which the generalisation was to be regarded as "confirmed" or "disconfirmed."

Professor Machlup goes on to describe this fundamental assumption of "maximising" or "rational" action, and its variants, as "assumptions which, though empirically meaningful, require no independent empirical tests but may be significant steps in arguments reaching conclusions which are empirically testable."

<sup>4</sup> Professor M. Friedman (*Essays in Positive Economics*, p. 16n) commenting on Professor Machlup's presentation of the marginal productivity doctrine (*American Economic Review*, Sept. 1946, pp. 519-54) notes that "in Machlup's emphasis on the logical structure, he comes perilously close to presenting the theory as a pure tautology, though it is evident at a number of points that he is aware of this danger and anxious to avoid it." I must say that Professor Machlup's "anxiety" on this point might well have seemed more pressing both in 1946 and in 1955. Anyone who was "anxious" could easily set about relieving his anxiety by giving an outline specification of the empirical content of the maximisation-of-returns hypothesis for the case in which he was concerned, that is, by indicating the conditions by which the hypothesis could be tested in a particular individual case. Professor Mises, of course, is not in the least "anxious" on this score: quite the reverse, he repudiates all anxiety by claiming that all economic action is "rational"—by definition presumably—and Professor Machlup seems at times most anxious to defend Professor Mises' position. We would note, in addition, that the point of view we are advocating is summed up very succinctly by Professor Friedman as follows (*op. cit.*, p. 41): "It is necessary to be more specific about the content of existing economic theory and to distinguish among its different branches."

It can certainly be agreed that actual independent tests may not be "required." But if one claims that a proposition is "empirically meaningful," or a "significant step," one *is* "required" to indicate where that significance begins and ends, what "work," if any, the proposition can and does do, and just why it is not a superfluous fifth wheel on the car—(as any such proposition as "all economic action, being rational, maximises whatever it maximises" certainly is).<sup>6</sup>

Of course it does not matter in principle whether the specification of the conditions of a test of this fundamental assumption is obtained "directly" and "independently," or by working back "indirectly" from the specified tests of the conclusions to the assumption from which the conclusions are deduced. According to Professor Machlup these conclusions *are* "empirically testable," that is, reasonably specific descriptions are available of what constitutes a test of them. What he does not show is how "empirically testable" conclusions about human actions can be deduced with logical inevitability from "empirically meaningful" assumptions about human actions, while these assumptions are to be regarded by themselves as either not conceivably, or not possibly, or not practically, or only "gratuitously" and "misleadingly," testable—which of these adverbs he really means, Professor Machlup never quite makes clear). In fact, what exactly is the contrast that Professor Machlup seems to be implying between "empirically meaningful" and "empirically testable," with regard to propositions about economic actions? At this point Professor Machlup rides off on analogies from physical theories without demonstrating that there is any relevant analogy in economic theory. In the social sciences there are, of course, considerable difficulties all along the line in testing *any* proposition. Professor Machlup does nothing to show that it is in any respect more difficult to confirm or "disconfirm" assumptions, "fundamental" or otherwise, about human actions in economic theorising, than it is to confirm or "disconfirm" the conclusions about human actions. The comparatively simple maximising theories of human action in micro-economics cannot in this respect relevantly be compared with the theories of physics. Methodological generalisations and analogies from physics are liable to be of rather limited significance in the interpretation and elucidation of specific

<sup>6</sup> Cf. the chapter 'The Analysis of Consumers' Behaviour' in *Welfare Economics*, by I. M. D. Little (p. 14 ff.). On the maximisation hypothesis as applied to the consumer, Mr. Little writes (pp. 20-21): "Where the chief difficulty lies is in the interpretation of the axiom 'the individual maximises utility'. . . . In the past economists have often been attacked on the grounds that their theories only applied to selfish people; such attacks were brushed aside as absurd. But they were not absurd. It was the economists who were wrong in suggesting that positive economics had any necessary connexion with satisfactions at all. Nor could the economist argue that he had some positive objective tests which showed him to whom the theory applied, and to whom it did not apply, and that it didn't matter whether it was really a test of satisfaction or not. He could not make this reply because no such test had been suggested. . . . One economist has tried to get over this difficulty by saying that it does not matter what a man tries to maximise, so long as he tries to maximise something, say his weight or his misery. But this amounts to a determination to say that whenever the economist can explain a man's behaviour then that man must be maximising something. It gives no indication whatever as to when the theory can be applied and when not."

economic theories and propositions.<sup>7</sup> In short, while admitting the principle of indirect verification, we cannot agree to the kind of loose and sweeping appeal to it which Professor Machlup seems to be making. Much more particularity and precision seems to be desirable.

Let us take the example where economists have for decades tried to draw the most sweeping and consequential practical conclusions from theories built round the fundamental assumption of maximizing or rational actions, that is, the theory of welfare economics and of consumers' behaviors. When we take a conclusion such as that of Walras (and many others) that "free competition procures within certain limits the maximum of utility for society," exactly the reverse procedure to that claimed as essential by Professor Machlup seems to be "required." It hardly seems very promising when confronted with such a "conclusion" to try to test it "directly." On the contrary, exactly reversing the process insisted upon by Professor Machlup, one must work back from the "conclusion" to the assumptions, and in particular the "fundamental" assumption about the individual consumer and producer, and enquire what would constitute a test of this fundamental assumption.

Again, Professor Machlup mentions, as a variant of the fundamental assumption, that "consumers can arrange their preferences in an order." How was this formulation arrived at and how did it come to replace for most economists (including, apparently, Professor Machlup) the earlier formulations in terms of "maximizing utility"? Simply thanks to the increasingly rigorous insistence by a long line of economists—(Fisher, Pareto, Slutsky, Hicks and Allen, Samuelson, and Little)—that the fundamental assumption of the theory of consumers' behaviour be testable.

When, on the other hand, Professor Machlup formulates the fundamental assumption to the effect that "people act rationally," it is not in the least clear what would constitute a test of this assumption and whether even it is testable. Not knowing how it can be tested, one cannot tell at all precisely what can be deduced from it. Nevertheless, brandishing this generalisation that all economic

<sup>7</sup> Cf. Little (*op. cit.*, pp. 2-4) on welfare economics: "In contrast to the undoubted validity of the formal deduction, what are called the foundations of the theory have always been shrouded in darkness. What are the foundations of a theory? The answer is, those postulates from which the theorems are deduced." In physics "it does not really matter in the least whether one believes that such words as 'electrons' and 'molecule' stand for entities of a peculiar kind, or whether one believes that they are merely words which serve a useful practical purpose. . . . But drawing analogies between physics and other studies can result in harm. . . . The analogy with physics breaks down in two important ways, which should lead one to suspect that what holds for one may not hold for the other. First, the concepts of physics about which people are not clear, do not appear in the conclusions. The conclusions are about macroscopic or microscopic objects, not about electrons. By contrast, in welfare economics, the conclusions are about welfare. Secondly, physicists' conclusions are verified or falsified; ours are not. . . . I do suggest that the reality of the theory (of welfare economics) has been badly overestimated by economists." These arguments apply to a lesser but none the less a very important extent—to the "maximising of utility" and the theory of the consumer, and even to the "maximising of profits" and the theory of the firm.

action was (or even must be) "rational" some economists—notably Professor Mises, whom Professor Machlup seems so concerned to defend—have proceeded to claim that wholesale political conclusions were logically deducible from it, and were thus to be regarded as established conclusions of economic science.<sup>8</sup> It is not difficult to understand why those wishing to propagate sweeping political dogmas as the established logical conclusions of scientific economic theory, should resist the claim that some procedure for testing should be described for these conclusions, and/or for the assumptions, including the fundamental assumption, from which they were deduced. I am afraid that Professor Machlup's doctrines on verification and verifiability in economics are not merely questionable in themselves as an account of the structure of micro-economic theory, but may be used in defence of a kind of politico-intellectual obscurantism that seeks to avoid not merely the empirical testing of its dogmas, but even the specification of what would constitute tests.

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#### REJOINDER TO A RELUCTANT ULTRA-EMPIRICIST

From the tone of Professor Hutchison's reply to my article I infer that he was hurt by my characterization of his position as one of ultra-empiricism. I am sorry that I hurt or angered him; I am glad that he rejects, at least on principle, the position which I called ultra-empiricism; and I am puzzled by many of his comments which still strike me as ultra-empiricist.

I agree fully with Professor Hutchison that his opening statement—on page 9 of his book and on page 476 of his note above—is a rejection of what I call ultra-empiricism. Whereas ultra-empiricists require direct empirical testing of propositions used as fundamental assumptions in a theoretical system, Professor Hutchison in this declaration seems satisfied with the *conceivable* testability of

<sup>8</sup> Cf. *Kritik des Interventionismus*, pp. 23-24, and *Liberalismus*, pp. 3, 78 and 170: "Liberalism is the application of the doctrines of science to the social life of men. . . . Liberalism and Political Economy were victorious together. No other politico-economic ideology can in any way be reconciled with the science of Catallactics. . . . One cannot understand Liberalism without Political Economy. For Liberalism is applied Political Economy, it is state and social policy on a scientific basis. . . . Liberalism starts from the pure sciences of Political Economy and Sociology which within their systems make no valuations and which say nothing about what ought to be or what is good or bad, but only ascertain what is and how it is. If this science shows that of all conceivable possible organisations of society only one, that resting on private property in the means of production, is capable of existing, because none of the others can be carried through, there is nothing in this which justifies the term optimism. . . . He who recommends a third type of social order of regulated private property, can only deny altogether the possibility of scientific knowledge in the field of Economics." Cf. also W. H. Hutt, *Economists and the Public*, p. 367: "Our plea is in short for that economic liberty which was dimly visualised by the Classical economists, and whose coincidence with the *summum bonum* has been an implication of the subsequent teachings of economic orthodoxy. We have attempted to show that expert, dispassionate and disinterested thought on these matters has been the preserve of those whose gropings in a world of divergent beliefs and arguments (beset on all sides by the lure of interests) have led them to the path of orthodox tradition."



the deduced *consequences* of these propositions. I might have quoted his statement on this point in support of my position that direct testing is not required—had he not in effect repudiated it by much of what followed it in his book. And he does it again in his note, as I shall attempt to show presently.

But do we really mean the same thing when we speak of "indirect testing"? Perhaps the crucial misunderstanding lies right here. Professor Hutchison mentions that a proposition not itself empirically testable directly must be "reducible by *direct* deduction to an empirically testable proposition or propositions." (Emphasis supplied.) This formulation suggests a requirement that the implications of any single proposition be tested independently of those of other propositions with which it is conjoined to constitute a "case." In fact, however, the *conjunction of logically independent propositions and derivation of their joint consequences* is the essence of indirect testing.

If assumption *A* can neither be subjected to any direct empirical test nor reduced "by direct deduction to an empirically testable proposition," its indirect verification can be accomplished by combining it with an assumption *B* which is directly testable; if a consequence *C* can be deduced from the conjunctive hypothesis *A plus B*—but not from either one alone—and if *C* is empirically tested, *A* is regarded as having passed the indirect test.

I suspect that Professor Hutchison does not accept the validity of indirect verification in this sense. Unfortunately, he makes no reference to my detailed exposition and schematic representation of the conception and operation of indirect testing. Silent on this, he professes to accept indirect testing and then proceeds to demand direct (independent) tests. I had pointed out that fundamental postulates, such as the maximization principle, are "not subject to a requirement of independent verification"; they are considered as verified, together with the whole theory of which they are a part, when the deduced consequences of their conjunction with an evident and substantive change and with assumed conditions relevant to the case are shown to correspond to observed events. Thus, if the fundamental postulate (e.g., that firms prefer more profit to less profit at equal risks<sup>1</sup>) is combined with assumptions about economic institutions and conditions (e.g., certain forms of competition) and with assumptions about certain substantive changes (e.g., the imposition of import quotas on certain products); and if we deduce from this conjunction of assumptions certain consequences (e.g., increases in the excess of domestic over foreign prices); and if these deduced consequences are found to be in relatively good correspondence with observed events (e.g., increases in the excess of domestic over foreign prices of bicycles) subsequent to actual changes of the kind in question (e.g., the imposition of import quotas on bicycles); then the theory is regarded as verified, and the fundamental postulate is regarded as verified with it.

Now, those who do not accept this "indirect verification" of the fundamental postulate but demand that the assumption of attempted profit maximization be

<sup>1</sup> On the problem of differences of risk and uncertainty in connection with differences in profits see my book *The Economics of Sellers' Competition* (Baltimore: Johns Hopkins Press, 1952), pp. 53-56.

empirically tested independently of the other propositions (about competition, import quotas, and bicycle prices) are the "ultra-empiricists" discussed in my article. If he understands this, I wonder whether Professor Hutchison will still deny membership in the society of ultra-empiricists or whether instead he will be eager to confirm it.

That Professor Hutchison misunderstands the essence of indirect verification is suggested by his example about the length of the unmeasured side of a triangular plot of land. If he had referred to the Pythagoras theorem as the general proposition in need of verification and to the lengths of the sides of his plot as the independently verifiable propositions, he might have come nearer to our problem, the validation of the use of universal propositions. What he really showed was (a) that he had confidence in the reliability of the Pythagoras theorem, (b) that he was sure his plot was reasonably close to a perfect right-angled triangle, and (c) that his measurements of the two short sides were reasonably accurate. The whole example has little to do with the question of the direct or indirect verification of fundamental assumptions employed in general theory.

That Professor Hutchison is not satisfied with the indirect verification of such universal propositions employed as fundamental postulates in general theory can be seen from several comments. For example, he contends (p. 478) that I have failed to state whether they are "conceivably falsifiable empirically" or rather definitions "without empirical content." (I had said they were "heuristic principles," "procedural rules," etc. See pp. 9 and 16). Then he demands (p. 478) "a specification of what a test would amount to, or of the more precise circumstances under which the generalization [of maximizing conduct] was to be regarded as 'confirmed' or 'disconfirmed.'" (I had stated repeatedly that the test consisted in checking the correspondence of observed events with the "assumed changes" and the "deduced changes" of the entire theoretical model. See especially p. 18.) It is quite obvious that Professor Hutchison, contrary to his initial declaration, wants more than indirect testing of the fundamental postulates of general theory.

Professor Hutchison asks whether my category of apriorism in economics is "so stretched to include all the middle ground up to the frontier line of 'ultra-Empiricism.'" (Since he also questions that I could name any "example of this category," he must believe that on my classification *all* economists are apriorists!) The answer is that I know very few "extreme apriorists" (e.g., Professor von Mises). The middle ground between the extreme positions is very large indeed; of the economists whom Professor Hutchison asked me to classify, it includes Zeuthen, Samuelson, Lange, and Friedman; none of them holds that no conceivable kind of experience could ever cause him to give up his theory, and none of them wants his fundamental assumptions empirically tested independently of the propositions with which they are combined when the theory is applied.

Professor Hutchison asks whether I can cite any other fundamental assumption in economics "beyond" that of "maximizing or rational action." It all depends on what one regards as fundamental. Perhaps the assumption that only limited outputs can be obtained from given resources should be called fundamental; it

"underlies" all economic problems, but it does not always become a relevant step in the argument. Perhaps still other (or narrower) assumptions should be proposed for inclusion, though frankly I had not intended it.

If the question referred to the possible replacement of, rather than addition to, the assumption of maximizing conduct, my answer would be that substitutes have been proposed, but not successfully. Some writers on the equilibrium of the firm (theory of output and price) have advanced "security of survival" and similar postulates in lieu of profit maximization (for the enterprise economy), but the proposed substitutes were less simple and less comprehensive. Yet, I grant the possibility that better postulates may be proposed, and therefore I have described the "Fundamental Postulates" as "Assumed Type of Action (or Motivation)" instead of limiting them to that of "maximizing conduct."<sup>2</sup>

In his comments on the nature and significance of the maximization postulate Professor Hutchison conveys the impression that he recognizes as scientifically legitimate only two kinds of statements: propositions which by empirical tests can, at least conceivably, be proved to be false, and definitions without empirical content. If so, he rejects a third category of propositions used in most theoretical systems: the heuristic postulates and idealized assumptions in abstract models of interdependent constructs useful in the explanation and prediction of observable phenomena.

Such propositions are neither "true or false" nor empirically meaningless. They cannot be false because what they predicate is predicated about ideal constructs, not about things or events of reality. Yet they are not empirically "meaningless," because they are supposed to "apply" or correspond broadly to experienced events. They cannot be "falsified" by observed facts, or even be "proved inapplicable," because auxiliary assumptions can be brought in to establish correspondence with almost any kind of facts; but they can be superseded by other propositions which are in better agreement with these facts without recourse to so many auxiliary assumptions.

Logicians have long recognized this intermediate category of propositions, which are neither *a priori* nor *a posteriori* in the strict sense of these terms.<sup>3</sup> (One may, with Friedman, prefer to say that a theoretical system has two parts, an analytical one demonstrating valid inferences, and a synthetic one stating correct applications.<sup>4</sup>) I had mentioned this category of propositions in my article (p. 16), but Professor Hutchison chose to disregard my remarks on this issue.

It was necessary to bring this up again because Professor Hutchison said

<sup>2</sup> The assumption of maximizing conduct of the householder may, of course, be broken down into several parts—that each person has preferences, that these preferences are consistent (transitive) and can be orderly arranged, that he wishes to follow these preferences in deciding on his actions, etc.—and it is possible to call each of these a separate postulate. This, I suppose, is not questioned here.

<sup>3</sup> They were called "procedural rules" by Felix Kaufmann, "complex-analytic propositions" by Wm. P. Montague, "constitutive, non-epistemic" propositions by Henry Margenau.

<sup>4</sup> Milton Friedman, *Essays in Positive Economics* (Chicago: University of Chicago Press, 1953), pp. 24-25.

(p. 478) that if I called the fundamental assumption (of maximizing behavior) "empirically meaningful" I should mean it to be "conceivably falsifiable empirically." I do not. Resolutions to analyse certain aspects of experience with the aid of a heuristic postulate, or even of a pure fiction, are not "falsifiable" but nevertheless "empirically meaningful."<sup>3</sup>

At another point (pp. 478-479) Professor Hutchison realizes that I did not mean that the fundamental assumptions about human actions should or could be empirically tested, and he asks me to show "how 'empirically testable' conclusions about human actions can be deduced" from those untested or untestable fundamental assumptions. I thought I had shown it with sufficient clarity; of course, the conclusions are deduced not from the fundamental assumptions in isolation but from their conjunction with other assumptions including some whose correspondence with factual observation is established.

I can easily comply with Professor Hutchison's request by pointing to the illustration I gave above, where I showed how a relative price increase for bicycles was the empirically testable consequence deduced from the partly untested or untestable assumptions. But Professor Hutchison repeats that I had done "nothing to show that it is in any respect more difficult to confirm or 'disconfirm' assumptions, 'fundamental' or otherwise, about human actions in economic theory, than it is to confirm or 'disconfirm' the conclusions about human action." Can there be any doubt that a direct empirical test of the motivations behind businessmen's actions, such as a test whether their decisions are made in an attempt to maximize profits, would be "more difficult," to say the least, than a test that higher prices are paid for bicycles?

Perhaps it was confusing when, in addition to stating that these fundamental assumptions *need not* be independently verified empirically, I also indicated that they *cannot* be so verified. Some economists who agree that no independent verification is required would none the less hold that such verification is *possible*; and others would contend that any special tests are *unnecessary* because the assumptions are *self-evident* statements of common experience. Common experience, however, tells us merely that we (that is, I and those with whom I have talked about it) *can* follow our preferences in choosing among the alternatives open to us and that we usually do it. Common experience, moreover, tells those of us who are or were in business that we *usually* attempt to make such decisions as would promise us the highest returns, but it does not tell us that *all* businessmen do so in *all* their actions. Indeed we know, also from common experience, that there are times when many businessmen refrain from following the most profitable courses of action and instead act to meet some demands of "patriotism" or to obey the moral suasion of governmental authorities. Are there any objective tests

<sup>3</sup> Some may wonder how one may possibly interpret the "fundamental assumptions" alternatively as rules of procedure (imperative statements), definitions (resolutions), useful fictions, and "true" empirical propositions. The answer lies in the convertibility of propositions. The following formulation may suggest how it can be done: "In analysing problems of this sort let us proceed by assuming that things will work as if businessmen were always attempting to maximize their money profits (and perhaps they actually do!)"

possible by which the assumption of profit maximization could be verified independently of the uses to which the assumption is put in economic theory?

We could conceivably place researchers into every business office to analyse every decision that is made and check the motivations behind it. This would not be quite reliable unless our researchers were invisible, had invisible lie detectors or perhaps mind-reading apparatus. In case we are satisfied with what is *practically possible*, we could have exceptionally competent and skillful survey researchers examine in carefully devised interviews a sample of the decisions made by a sample of businessmen. The object would be to establish the relative frequency of decisions consistent with profit maximization: In what percentage of their decisions do businessmen believe that they are acting in the best (long-term) interest of their firm (that is, of its owners)? Surely, some businessmen do so some of the time; probably, most businessmen do so most of the time. But we would certainly not find that all of the businessmen do so all of the time. Hence, the assumption of consistently profit-maximizing conduct is contrary to fact.

Of course, no proposition about empirical facts can be absolutely certain; but here we are defending an assumption of which we are certain that it does not always conform to the facts. If the deviations are insignificant we can safely neglect them. But we do not know *how* significant they might be, especially because the relative strength of non-profit objectives changes with the conditions of the time, changes probably also with the kind of decisions, and changes perhaps also with several other factors. What then should be done? Just what is being done: to accept maximizing conduct as a heuristic postulate and to bear in mind that the deduced consequences may sometimes be considerably out of line with observed data. We can, to repeat, test empirically whether the outcome of people's actions is most of the time reasonably close to what one would expect if people always acted as they are unrealistically assumed to act. Again, the "indirect verification" or justification of the postulate lies in the fact that it gives fairly good results in many applications of the theory.

Professor Hutchison has several questions concerning the assumption of maximizing conduct; we shall call it for short the Assumption (with capital A). He asks (a) "just what content, if any, it has been meant to possess," (b) "just when, where, and how far it is applicable," (c) "what a test [of it] would amount to," (d) under what circumstances it "was to be regarded as 'confirmed' or 'disconfirmed,'" (e) "And he finds that I am 'required' to indicate (e) the range of the 'significance' of the Assumption, (f) 'what 'work,' if any,' it can do, and (g) 'just why it is not a superfluous fifth wheel on the car.'" I shall attempt brief answers to all seven questions.

(a) I am not sure what sort of "content" it is that is in question. Does "content" refer to specific data of experience that have gone "into" the Assumption and are now an integral part of it, as in the case of a universal proposition whose subject can be defined by complete enumeration? In this sense the Assumption has no determinate "content." Or, rather, is the question whether the Assumption is to apply to empirical data of a certain class, and whether it would matter if it did or did not apply? In this sense the "content" of the assumption of profit



maximization can readily be illustrated. Suppose (1) the government announces that price reductions would be in the national interest, (2) wage rates have just been raised, (3) raw-material prices have gone up, (4) no changes in technology have occurred for many years, and (5) aggregate demand has not changed. Should we expect product prices to rise or to fall? If firms did not attempt to maximize profits, they might well act in accordance with what the government publicizes to be in the national interest, and prices would be reduced. The Assumption does make a difference.

(b) The applicability—"when, where and how far"—of the Assumption, or rather of theories based on it, can be "prescribed" in broadly formulated directives, but there will always be a wide margin for the use of good judgment. The "directions for use" may be different for explanations of past events and for predictions of future events. In general, for purposes of prediction, we should *not* apply the Assumption to particular households or to particular firms, but only to large numbers of households or firms, or rather to cases where the deduced events, such as changes in prices, outputs, consumption, exports, imports, etc., are regarded as the outcome of actions and interactions of large numbers of firms and households. We should apply it only with reservations in times when strong moral suasion is exerted to make people disregard their usual preferences or interests, such as in war time when patriotic objectives are strongly pressed.

(c) Our discussion of the "kind of test" to which the Assumption should be subjected has probably been sufficient to warrant our conclusion that the test of the pudding lies in the eating and not in its ingredients. If we find no better theory to explain or predict changes in prices, outputs, etc., etc., and if our present theory does no worse than it has been doing, we may consider our Assumption as warranted.

(d) The Assumption will of course never be considered as "confirmed" for good, but only until further notice. Under what circumstances is the Assumption to be regarded as "disconfirmed"? When a theory not using this Assumption is proposed and is shown to work equally well for a wider range of problems, or with a smaller number of variables or provisos, or more reliably or more accurately for the same range of problems and with the same number of variables or qualifications—then the Assumption will have outlived its usefulness and will be sent to the limbo of "disconfirmed propositions." (And even this need not be beyond recall.)

(e) May I take the "range of the significance" to mean the same thing as the "when, where, and how far" of the applicability of the Assumption? If so, I may refer to what I said under (b). These answers, however, are strictly confined, as was my article, to positive economics, that is, to explanations and predictions of economic changes and events. Normative or evaluative economics has been outside the scope of my discussion; hence, I am not examining the significance of the Assumption for welfare economics. To give a simple example, we have been concerned with questions like "what consequences can be expected from the removal of a tariff," not with questions like "whether these consequences would be desirable" and "whether the tariff ought to be removed."

(f) The kind of "work" the Assumption does for us was indicated under (a), where its "content" was discussed.<sup>8</sup> Let me add two more illustrations, (A) from the theory of the household, and (B) from the theory of the firm and industry. (A) Suppose (1) the tastes for foodstuffs are given, (2) the substitutability between vinegar and lemon in salad dressings, the complementarity between salad dressings and salads, and the income elasticities of demand for both are all given with the tastes, (3) the prices of lettuce and other salads are reduced, (4) disposable incomes rise, and (5) the price of vinegar is increased. If we trust the Assumption we can predict increased consumption or increased prices of lemons (or longer queues if lemon prices are fixed, and more bootlegging if lemons are rationed). Without the Assumption we cannot say anything, for if people do not follow their preferences, act inconsistently and haphazardly, "given" scales of preference mean nothing. (B) Assume (1) the technological conditions of production are given, (2) entry into the textile industry is open, (3) the supply of productive services required for textiles is elastic, and (4) the demand for grey goods increases. On the basis of the Assumption we can explain or predict a larger output of grey goods; without the Assumption we cannot. If businessmen like smaller profits just as well as bigger profits, or even better, why should any manufacturer increase his output when demand increases? If businessmen are not tempted by opportunities to make more profit, why should anybody take up the production of grey goods? It is hard to understand how any doubt can be entertained as to "what work" the Assumption does for us.

(g) The question whether the Assumption is not really "superfluous" is, I believe, disposed of with our description of the "work" it does for us. To be sure, the same work might possibly be done by a different assumption—and we know that many versions of the fundamental "Type of Action" have been used over the years—but I doubt whether the difference can be very great. But while the Assumption might be replaced by an alternative, it cannot be eliminated without replacement; it is not a redundant part in the theory. It is perhaps possible to put an indefinite number of "behavior functions" in the place of our Assumption, with the stipulation that all consumers will consistently stick to these functions. Such a stipulation would be neither simpler nor more realistic than the Assumption; and since the required knowledge of all behavior functions would be a heavy burden for the theory of consumer behavior, this whole approach is distinctly inferior to the traditional theory. The latter has yielded a large number of generalizations as "deduced consequences" even without knowledge of the exact preference systems of consumers, merely on the basis of some very general properties of such preference systems. As for the theory of production in an enterprise economy, the Assumption appears to be indispensable. Never could a behavioristic approach provide all the millions of "entrepreneurial behavior functions" which would be needed to do the job that is now done by the simple postulate of profit maximization.

A few minor misunderstandings remain to be cleared up. The assumption

<sup>8</sup> I prefer to speak of "the work it does" rather than of "the content it has"; both are metaphors, to be sure, but the latter, I think, is quite infelicitous.

that "consumers can arrange their preferences in an order" is not, as Professor Hutchison believes (p. 479), a "variant" of the fundamental assumption, "replacing" earlier formulations in terms of "maximizing utility." Nor has it been proposed "thanks to the insistence" of Hicks and Allen, Samuelson, and Little to make the theory testable. Instead, the phrase was used by Robbins<sup>7</sup> and can be traced back to Čuhel<sup>8</sup> and the earlier Austrians; and it was proposed in order to spell out the logical prerequisites of maximizing utility.

In a footnote (p. 481) Professor Hutchison approvingly quotes I. M. D. Little concerning certain differences between physics and economics in the use of fundamental assumptions. One of the differences singled out for emphasis is supposed to be that the "concepts . . . about which people are not clear"—pure constructs, idealizations, and postulates—"do not appear in the conclusions" in physics, but do so in "welfare economics." I have not discussed welfare economics and do not intend to do so. But that the controversial, "untested" assumptions "do not appear in the conclusions" holds, as I have demonstrated, for positive economics no less than for physics.

In another footnote (p. 480) Professor Hutchison believes that he has found an ally in Professor Friedman, who held that I had come "perilously close" to a tautological formulation of the theory. But by pressing his demand for an independent empirical test of the profit maximization postulate Professor Hutchison has placed himself right in the center of the target of Friedman's attack. It was the main theme of Friedman's methodological essay that fundamental assumptions do their work even if they are contrary to fact, and that it is a mistake to attempt empirical tests for them besides those of the findings derived from the theory of which they are a part.

There is, furthermore, the charge of "tautology," which is implied in some of Professor Hutchison's strictures against my work and is made explicit in the quotation from Friedman. The judgment that a certain theory is "purely tautological" may mean rather different things: that the theory is underdetermined and can yield no specificable conclusions; that some of the important variables are unknowable or changing in undetermined ways; that the *ceteris paribus* clause is used without specifying the *cetera* or their significance for the outcome; that the deduced conclusions can never be tested against data of experience; that the theory constitutes an internally consistent and closed system; that some of the assumptions are "empirically empty." I shall comment here only on the last two meanings of the charge.

A fully developed theoretical system will always be "an internally consistent set of assumptions and definitions, such that each proposition is capable of being logically deduced from the assumptions and definitions (in the manner of a theorem)." This was, and probably still is, recognized by Professor Hutchison,

<sup>7</sup> Lionel Robbins, *An Essay on the Nature and Significance of Economic Science* (London: Macmillan, 1932), pp. 56, 86, and elsewhere.

<sup>8</sup> Franz Čuhel, *Zur Lehre von den Bedürfnissen* (Wien, 1908), pp. 186-216.

<sup>9</sup> Arnold M. Rose, *Theory and Method in the Social Sciences* (Minneapolis: University of Minnesota, 1954), p. 263.

who once wrote that pure theory must necessarily be of a form such that "what it proves must be contained in the assumptions and cannot be obtained from any other sources." Hence, "to criticize a proposition of pure theory as such as tautological, or circular, or as assuming what it requires to prove, is beside the point."<sup>10</sup>

The assumptions that consumers act to "maximize their expected satisfaction" and entrepreneurs act to "maximize their expected profits" are sometimes considered as "empirically empty" or "tautological" because (a) we cannot know whether or not the consumers and entrepreneurs really believe that their actions are the best of the alternatives considered, (b) whatever they do can thus be interpreted as being what they consider to be "the best under the circumstances," and (c) as long as we do not know their tastes, preferences, and alternative anticipations, we cannot deduce any particular way of acting from the assumptions standing by themselves.

The point, however, is that the assumptions do not stand by themselves but are combined with other assumptions, including some about certain substantive changes which are observable by us as well as by the consumers or firms concerned. Our theory does not tell or explain what the decision makers have been doing, or have preferred to do, or have avoided to do before the changes in question occurred; it deals only with the ways in which decisions will be changed by the occurrence and its repercussions. No matter how many pounds of lemons consumers have been purchasing, they will try to purchase more; the theory tells us this from the assumptions furnished. No matter how many yards of grey goods manufacturers have been producing, they will produce more; the theory can tell us this on the basis of the assumptions supplied.<sup>11</sup> An assumption apparently quite "empty" or without empirical implications as long as it stands alone may become of definite empirical significance when combined in a model with other assumptions.

Finally, there is that polemical red herring dragged across the trail: veiled charges of sympathizing with controversial value judgments, "indirect" accusations based on guilt by association with others accused directly. I was first inclined to overlook it, because I thought that silence on my part would be the most eloquent response. I have been persuaded, however, that my rejoinder would be sadly incomplete without a comment on this confusion, innocent or deliberate, between positive economics and political evaluation.

Not a single passage or sentence in my article could in fairness be interpreted as dealing with political implications, value judgments, policy advice, welfare economics. Yet, in the last pages of his reply Professor Hutchison throws a heavy barrage against alleged welfare implications of my argument. Furious salvos are

<sup>10</sup> T. W. Hutchison, *The Significance and Basic Postulates of Economic Theory* (London: Macmillan, 1938), p. 36.

<sup>11</sup> See my reply to R. A. Gordon, who had interpreted methodological subjectivism as leaving "theory saying that businessmen do what they do because they do it." Fritz Machlup, *The Economics of Sellers' Competition* (Baltimore: Johns Hopkins Press, 1952), p. 36.

fired against the "maximum of utility for society" in connection with Walras and free competition, and against "wholesale political conclusions" in connection with Mises and liberal economic policies.

If Professor Hutchison really believes that my "doctrines on verification and verifiability" can be used (and are designed?) to "propagate sweeping political dogmas" and to defend "politico-intellectual obscurantism" he does precisely what he apparently considers objectionable in others: he confuses normative (ethical) judgments with positive propositions of economic theory. Yet, at the same time he claims to be an advocate of Professor Friedman's dictum that "It is necessary to be more specific about the content of existing economic theory and to distinguish among its different branches."<sup>12</sup> Would that Professor Hutchison practiced what he advocates.

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Fritz Machlup

#### A NOTE ON TESTING THE TRANSITIVITY AXIOM

Recently Pfouts<sup>1</sup> in discussing the testing of certain fundamental assumptions in the theory of ordinal utility has stated "that if we can show empirically that the transitivity rule is not obeyed by individual consumers then there is something wrong with the existing theory of consumer's preferences." Pfouts cites the results of May<sup>2</sup> as preliminary evidence indicating a tendency toward non-transitivity in human choice.

It is the purpose of this note to clarify the conditions under which the transitivity axiom would be refuted and to make clear the type of tests under which the axiom could never be refuted. The transitivity condition stated in Pfouts' terminology is that if  $X_i R X_j$ , and  $X_j R X_k$ , then it must follow that  $X_i R X_k$ , where  $R$  indicates the relation "preferred or indifferent to." But we must recognize that in ordinal utility theory  $X_k R X_i$  and even  $X_j R X_i$  are also allowable relationships provided that all the  $R$ 's in the relations represent the condition of "indifference." In fact in ordinary utility theory one can use the definition that indifference exists between  $X_i$  and  $X_j$  if and only if  $X_i R X_j$  and  $X_j R X_i$ .

We are now able to appreciate that any binary choice experiment, and May's in particular, in which the individual being tested *must* make a choice between  $X_i$  and  $X_j$  only demonstrates the relation  $R$ ; the existence of a cyclical pattern  $X_i R X_j R X_k R X_i$  in no way contradicts the axiom of transitivity. An equivalent statement of the transitivity condition to be tested which pinpoints the critical test to be made is that if  $X_i R X_j$ ,  $X_j R X_k$ , then not  $X_k P X_i$ , where  $P$  indicates the relation "preferred to."

We may easily demonstrate by an intuitive approach that cyclical choice patterns of  $R$  are admissible. Consider an individual who is indifferent to  $X_i$ ,

<sup>12</sup> Milton Friedman, *op. cit.*, p. 41. Emphasis supplied.

<sup>1</sup> R. W. Pfouts, "Prolegomena to the Testing of Utility Theory," *Southern Economic Journal*, Vol. XXII, October 1955, pp. 178-188.

<sup>2</sup> K. O. May, "Transitivity, Utility and Aggregation in Preference Patterns," *Econometrica*, Vol. XXII, January 1954, pp. 1-13.



$X_j$ , and  $X_k$ . Forced to make binary selections between  $X_i$  and  $X_j$ ,  $X_j$  and  $X_k$ , and  $X_k$  and  $X_i$ , the individual elects to toss an unbiased coin. A cyclical choice pattern may of course result.

Our conclusion is that tests for verifying the transitivity axiom must be of a nature so as to distinguish between and not confound the relations of "preferred to" and "indifferent to."

Pfouts suggests that intransitive binary choice patterns may occur if the pattern is truly a family preference pattern. We still might ask whether it is possible to give a reasonable preference aggregating function for an individual such that intransitive preferences are indicated. The following function is suggested for the case of three alternatives-three criteria (and of course will generalize for larger sets): the individual makes his selection according to a single criterion except when the other two criteria *both* give counter indications. Such a function seems reasonable from the point of view that often an individual has a "major criterion" of preference in mind, but will yield to the "minor criteria" provided they are unanimous in their dissent. In May's experiment with college students, the intransitive choice pattern  $xyzx$  is perhaps explained by an underlying pattern in which "intelligence" is the major criterion and "looks and wealth" the minor criteria.

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HARVEY M. WAGNER

## MEMORIAL

JOHN BROOKS WOOSLEY

1892-1956

John Brooks Woosley, Kenan Professor Emeritus of Finance in the University of North Carolina and President of the Southern Economic Association during 1939-1940, died in Durham, North Carolina on January 21, 1956. He was born at Ashboro, North Carolina on January 20, 1892, the son of a Methodist minister. This meant that he grew up in many places with varied experiences, but anchored in a home where thrift, high moral standards, clean living and close family loyalties were woven into his character.

Dr. Woosley's education was obtained in the rural and village schools of the Piedmont section of North Carolina. Entering Guilford College in 1908, he soon became a leader in student affairs with particular interest in debating and athletics. These activities, however, were wisely adjusted to his academic duties as attested by his graduation with high honors with an A.B. degree in 1912. He continued his education at Haverford College in Pennsylvania in 1912-1914 where he received his M. A. degree.

The teaching profession appealed to him, so in September, 1914 he began what was to be a long and distinguished career in that field. He was appointed instructor in history and social science on the staff of his Alma Mater, Guilford College. In 1917, like most young men of his age, he entered the service of his country in World War I. After obtaining his commission at Camp Jackson in the summer of 1917, he served in the French combat zone as a lieutenant in a machine gun battery. Upon his discharge from the Army, he found a teaching position at once available as principal of Jamestown High School in Guilford County, N. C. He always valued this experience since it gave him an insight into secondary school methods and standards.

In September 1920, he came to Chapel Hill as a member of the growing group of young men in economics and business administration who were developing a program in business training and an expanded offering in economics both at the undergraduate and graduate levels. He continued his graduate work in summers at Columbia and Chicago Universities. Granted a leave of absence, he completed his doctorate at the University of Chicago in 1931.

Professor Woosley advanced rapidly in academic rank to a full professorship in 1931. In recognition of his outstanding qualities as a teacher, researcher and administrator he was chosen in 1951 for the distinguished rank of Kenan Professor of Finance. His constant and intense devotion in his work and his students finally undermined his health and led to his retirement in September, 1953.

In evaluating the life of this beloved and able man, many outstanding things come to mind. He was, first of all, a great teacher, who never allowed his teaching to become routine. His classes were a stimulating, challenging and inspiring

experience for his students who in after years remembered him with high respect and warm affection.

Dr. Woosley's research in the field of banking was widely recognized for its clarity of analysis, careful factual support and outstanding constructive value in current banking problems. His pioneer work in developing the North Carolina Bankers Conference with its annual week of intensive study at the University of North Carolina became a pattern which by contagion has spread to most of the other states of the Union. This movement, along with his "Trends in Banking," has probably done more to raise the level of performance of small banks in the Southeast than any other agency.

As a colleague, Dr. Woosley, while not an extrovert, had those rare qualities of good humor, sparkling good will and warm fellowship which bound his associates to him with deep devotion and high respect. His friendship was a great privilege and his memory will be cherished by all who knew him.

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D. D. CARROLL

## BOOK REVIEWS

*The Psychology of Economics.* By Walter A. Weisskopf. Chicago: University of Chicago Press, 1955. Pp. viii, 266. \$4.00.

Although little recognized until the turn of the century, it is now a commonplace that psychological assumptions and personal complexes have greatly influenced the theories of economic motives and behavior which have been formulated by the leading economists through the ages.

The dominant assumption in the framing of economic doctrines throughout the nineteenth century was that man is a purely rational being, carefully calculating his actions to secure the greatest possible amount of satisfaction and to avoid annoyance, pain and misery, so far as possible. This theory was best consolidated and expressed by Jeremy Bentham, the leader of the Utilitarians, in his famous "felicific calculus." It guided economic thinking down through the days of John Bates Clark, Frank A. Fetter and Herbert J. Davenport. The same postulate about man and his rational behavior controlled social and political thought as well.

The first important revolt against rationalism as a working hypothesis in the social sciences came with the rise of social psychology in the writings of Walter Bagehot, Gustave Le Bon, Scipio Sighele, Gabriel Tarde, Edward A. Ross and others. The first notable attempt to utilize this new frame of reference as a frontal attack upon the rationalistic basis of social thinking appeared in Graham Wallas' *Human Nature in Politics*, published in 1908. Wallas made it clear that human behavior and social thought are not solely the product of rationalistic self-interest, but are greatly influenced, if not determined, by such factors as instincts, custom, tradition, mass psychology, crowd behavior, and deliberate propaganda.

The significance of all this for the proper appraisal of economic behavior and theory was first fully accepted by Wesley Clair Mitchell in his article on "Human Behavior and Economics," published in the *Quarterly Journal of Economics*, in 1914. He followed up the argument in his famous article on "Bentham's Felicific Calculus" in the *Political Science Quarterly*, four years later. Much interest in this new approach was generated by Carlton H. Parker's challenging paper on "Motives in Economic Life," delivered before the American Economic Association in 1917. His ideas and position were supported and amplified by William F. Ogburn. Notable also in this general period as challenging the rationalistic hypothesis were such works as Veblen's *Instinct of Workmanship*, Helen Marot's *The Creative Impulse in Industry*, Ordway Tead's *Instincts in Industry*, and Z. C. Dickinson's *Economic Motives*. The reviewer was made keenly aware of this new trend in economic thinking and interpretation, because he was during a part of the time a colleague of Mitchell, Veblen and Wallas, and attended their lectures as well.

Those who seek or expect any elaboration of this approach to "the psychology of economics" in Professor Weisskopf's book will be disappointed. The names of

Mitchell, Veblen, Parker, Ogburn, Marot, Tead, Dickinson, and the like, do not appear in the index. The only economist listed who is notable for his attack on the rationalistic approach is Vilfredo Pareto, and he is mentioned only casually in a footnote.

Professor Weisskopf is primarily concerned with discovering what psychological complexes, compulsions and symbols may have played a leading role in determining the economic thinking of Adam Smith, David Ricardo, Thomas R. Malthus, Karl Marx, and Alfred Marshall. He relies for the most part upon "psychodynamic" (mainly psychoanalytic) and "sociocultural" techniques and modes of interpretation. Symbolism, chiefly sexual, replaces both rationalism and social psychology, as the main tool utilized.

Male and female symbolism is held to have dominated the basic economic thinking not only of Ricardo and Malthus, but also of their main critic, Karl Marx. In the case of Ricardo and Malthus, land symbolized the female and labor the male. With both of these writers, land (female) was the root of all (or most) evil. In Malthus' thought, land's niggardly inability to maintain productivity on a par with population growth was the leading cause of human misery. Population growth, itself, is the result of the menace of female fecundity, implemented by male response to the fatal sexual lure of women. Ricardo believed that a main evil was the rent which land commanded and thus exacted a disproportionate and unjustified fraction of the economic income. His labor theory of value grew mainly out of his rationalization of the superior and benign nature of the male, symbolized in the economic field by labor—a father identification.

The leading critics of classical economics and economic liberalism, notably, Marx, also capitulated to this sexual symbolism in their analysis of economic motives and behavior, but they introduced a new angle or "twist." This is most clearly manifested in Marx's hypothesis of surplus value, which Professor Weisskopf interprets in terms of the Freudian Oedipus complex. Although, in the Marxian symbolism, labor is still the dominant item in production, it no longer symbolizes the father but the son. The latter is in rebellion against the father who seeks to deprive the son of access to his mother (and to sex) and threatens the son with castration. In Professor Weisskopf's words:

All the elements of this situation can be found in Marx. According to Freud, the son is hostile to the father, because he takes something away from him: the mother, the female in general through the injunction against premarital sexual activity, and the penis through castration as punishment for the violation of these taboos. According to Marxian theory, too, something is taken away: the means of production and the surplus value. Like the Freudian son, who considers the mother as his property to whom the father bars his access, the Marxian labourer considers the means of production and the surplus value to be his: it is he who produces both, and, therefore, they are supposed to be rightly his. The Freudian conflict is a conflict about property rights to the mother or the female in general. The Marxian conflict is a similar conflict about property rights to the means of production and to the surplus value.

This is all very interesting and surely gives a new and colorful "lift" to the discussion of economic theory. One can well imagine the reaction of austere



gentlemen of the "old school" like John Bates Clark, J. Laurence Laughlin, and Charles J. Bullock to such an interpretation of the genesis of the Marxian idea of surplus value, although it may not be further from fact and reality than Clark's theory of distribution.

The reviewer, having been one of the first social scientists in this country to recommend the application of dynamic psychology and psychoanalysis to the interpretation of the opinions of statesmen, publicists and scholars, is naturally sympathetic with Professor Weisskopf's approach. But he must register a robust doubt as to whether he has used the right methods. One can surely gain only very limited insight into the complexes and compulsions of a personality from his writings. Even a careful study of all the known facts about the life of men so remote in time as Smith, Ricardo and Marx would give us only an imperfect and inadequate comprehension of what motivated their personal drives and theoretical postulates. But it would furnish a better basis for judgment than the more or less impersonal imposition upon these men of some highly generalized "Jungian" types of symbolism or universalized Freudian mechanisms. The father identification and the Oedipus complex, for example, may have considerable validity, but there is no evidence that Professor Weisskopf has ascertained the extent to which they actually may have affected the life or theories of Ricardo, Malthus or Marx. In short, he has called attention to a possibly fruitful approach rather than carried it through to successful execution, so far as the limited data would permit.

The treatment of Smith and Marshall fails almost entirely to live up to the theme and method announced in the title of the book. There is much astute conventional analysis of their economic doctrines, but it is hardly original, much less an explanation of their opinions in the light of underlying subconscious influences. Smith's optimism and inconsistencies and his emphasis upon labor, and Marshall's rationalism, ameliorism, and eclecticism have been pointed out time and again by writers on the history of economic theory.

The final section of the work deals with "The Disintegration of Economic Rationalism," but we have already pointed out the amazing fact that none of the men who took the leading part in exposing the fallacies of economic rationalism: Wallas, Mitchell, Veblen, Parker, Ogburn, Dickinson, Tead, and others, are mentioned anywhere in the book, to say nothing of this section in which recognition of their work is indispensable.

It may not be too harsh a judgment to state that where the book is sound but conventional, its contribution is not needed, while in those portions where it is unconventional it is also unconvincing. Yet, the reviewer will cheerfully concede that, if one hesitates to state obvious facts in obvious language, then the transposing of conventional economic dogmas into sexual symbolism is a more lively and intriguing mode of obfuscation than obscuring the obvious in terms of calculus and higher differential equations. In so far as Professor Weisskopf's methodology is a revolt and protest against the all-pervading quantitative mysticism in recent economic writings, it is to be hailed and commended.

*Malibu, California*

HARRY ELMER BARNES

*Economic Theory and Method.* By F. Zeuthen. Cambridge, Mass.: Harvard University Press, 1955. Pp. xii, 374. \$6.00.

This is a translation in a revised form of a book that originally appeared in Danish in 1942. The author insists that "this book does not aim at providing a summary of the most important contributions that have been made by economics towards the solution of practical problems. Essentially, the purpose is only to treat one type of contributions: those of economic theory" (p. 3). The book comprises four sections: Part I, Method and Theories; Part II, Economic Life—A Network of Partial Interdependencies; Part III, Dynamics; and Part IV, Forms of Market and Structure of Society. The following topics, chosen at random from the many discussed in this work, indicate the breadth of its coverage: the Walrasian system of equilibrium equations, the cobweb theorem, the Swedish theory of processes of contraction and expansion, bilateral monopoly, the effects of advertising, the theory of games, national income analysis, the measurement of utility, spatial competition, input-output analysis, property and power, non-price competition, and economic warfare. Virtually all of the topics usually included in an intermediate theory textbook will be found here, and the author explains patiently if briefly such things as why the average total cost curve is U-shaped, how to read indifference curves, and the nature of isoquants, although this is not intended as a textbook and it presupposes some knowledge of economic analysis. The author demonstrates an impressive familiarity with the literature, including works by American and British writers, and the coverage is so broad that most readers will find some things which are new along with much that is familiar.

It was not the author's intention in this book to make contributions to economic theory, although references are made to some of his original work which has appeared elsewhere. This is a book about economics—specifically about the method of economics. And the method of economics is explained by summarizing the many devices and techniques which have been used by economists in the solution of a wide variety of problems.

Zeuthen begins with the assumption that it is the function of economics in general and economic theory in particular to assist in the solution of practical problems in the real world. Economics must be more than mental gymnastics; it is a science, and it is more than an extension of mathematics or philosophy. The success of economic analysis must be measured in terms of its usefulness in the solution of real problems. An economic theory whose assumptions are not in close accord with the real world, it is argued, cannot be expected to be helpful in the solution of practical problems. This requires that the economist be familiar with many models corresponding to the diverse situations met in the real world, and he must acquire a degree of proficiency in inventing new models to help in the solution of problems for which the old analysis is inadequate. Many kinds of theories are required, as is suggested by the following classification: total vs. partial; homogeneous vs. non-homogeneous quantities; economics of society as a whole vs. the economics of the firm; static vs. dynamic; stationary vs. changing; short run vs. long run; models vs. description; central theory vs. branch theories; and general vs. special theories (Chapter 7). All of these and more are required.

A perusal of this book is likely to leave the reader with two curious impressions: (a) how much we expect our graduate students in economic theory to learn, and (b) how little economists really know. There seems to be a danger that, if our models become so numerous and complex, our map will be scarcely less detailed than the terrain which it describes; and a map is useful only because it leaves much out of the picture. Zeuthen is unquestionably right in that economists have attempted to solve each new problem with a new model, and a general survey of our discipline, such as this, dramatically calls attention to the proliferation of types of theories which has taken place in recent decades. Zeuthen assumes that this is proper and, presumably, inevitable. The ultimate will be reached, one supposes, when the economist's network is so complete that any problem arising in the real world can be properly classified and the appropriate analysis will be available, perhaps by punch-card.

It may be that this is the inevitable direction of progress in economic analysis. But an alternative needs to be considered. Is it axiomatic that "whether the result and conclusions (of a theory) are *really* true depends on the reality of the assumptions on which we have built" (p. 7)? There is never a perfect correspondence between our assumptions and reality. Isn't the important question whether the theory is useful in prediction? If a market is less than perfectly competitive but the competitive assumption gives us an answer that corresponds with reality, is the theory of no value in this use? How much better do the tools of monopolistic competition make our analysis of practical problems than it would be if we classified all markets as being either perfectly competitive or less than perfectly competitive? What does the economist know that would be of help to the businessman or statesman that does not grow out of comparative statics? Has the work done to date in dynamics actually increased our usefulness as economic advisors?

These questions are not raised by Zeuthen. The asking of them does not imply what the right answers are. But it is curious that we should incorporate into the corpus of standard economic theory the many excursions which economists have properly made into the never-never land without giving these basic questions the same thorough consideration which characterizes our analysis of the economic problems themselves.

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CLARK LEE ALLEN

*Quality and Competition: An Essay in Economic Theory.* By Lawrence Abbott. New York: Columbia University Press, 1955. Pp. ix, 229. \$3.75.

This work by Professor Abbott is likely to prove a substantial contribution to economic theory. It is an exceedingly well written and well thought out investigation of an important deficiency in contemporary economic theory—the lack of a theory of quality in the area of nonprice competition. This book is a relatively successful attempt to weld quality theory and price theory into a unified theory of competition.

The study is organized in three sections: Part One, "Quality Variability and Existing Economic Theory," describes the present state of the theory of competition and its incompleteness; Part Two, "The Theory of Choice of Quality," introduces Professor Abbott's theory of quality competition; Part Three, "The

Quality Variable and Competition," brings quality theory together with price theory to form a more complete general economic theory of competition than now exists.

Professor Abbott points out that while price theory has served economists for many years, it fails to explain completely the phenomena of the market place, since people in making purchases relate price to quality; not only are there various alternatives as to products, but there are alternatives as to price and quality within any given group of alternative products. Thus, a complete theory of market competition should embrace the price aspects and the quality aspects of the products. To arrive at a theory of quality, Professor Abbott has organized the wants of people into two classifications: basic wants, which he defines as an individual's desire for an experience; and derived wants, an individual's desire for a product which may make that experience possible. He then adopts the concept, "constellations of wants," which he defines as "a complex of related supplementary wants, usually consisting of a major want plus numerous minor wants," which incidentally leads him to a closely-defined functional definition of an industry.

Professor Abbott's study of wants develops into a discussion of consumers' and producers' behavior and allows him to make a number of cogent observations concerning the market. His conclusions as to consumers' and producers' behavior form the basis for a number of propositions concerning the characteristics of competitive markets. Professor Abbott's theory of quality consists of these conclusions as to consumers' and producers' behavior in the market place, as they are influenced by their wants and, therefore, by the qualities of various products which will satisfy those wants.

An important part of this work is Professor Abbott's careful and systematic definitions of terms: for example, noneconomic vs. economic competition, price vs. nonprice competition, and the distinction between price and quality competition. He draws careful distinctions among the kinds of quality competition: Vertical competition arises from the kind of quality comparisons which may be described in terms of higher or lower quality. Horizontal quality competition arises from differences about which there is no clear-cut agreement, as is the case with individual color preferences. Innovational quality competition arises from differences of quality in which one product is "improved" over another.

Professor Abbott has made a valiant attempt at combining price competition and quality competition into a more complete theory of competition. He describes market behavior under so-called "ideal" conditions somewhat equivalent to the "pure competition" ideal of the price theorist and concludes, like the price theorist, that a description of market behavior under such ideal and complete competition is unreal. He, therefore, determines at the end that a situation which he describes as "incomplete competition," which is a market in which both price and quality considerations influence buyers and sellers, but in which imperfect knowledge exists—in short, an equivalent to the "imperfect competition" or "monopolistic competition" of the price theorist—is the general case and should, therefore, provide the theoretical description of the actual market.

This book is a pleasure to read; Professor Abbott should be congratulated on his lucidity in a piece of work which could have been difficult of comprehension.

*Louisiana Polytechnic Institute*

HOWARD L. BALSLEY

*American Imports.* By Don D. Humphrey. New York: Twentieth Century Fund, 1955. Pp. xviii, 546. \$6.00.

The argument of this book may be summarized as follows: In spite of substantial reductions in the American tariff—"the realized average duty on dutiable imports was 47 per cent in 1934 and 14 per cent in 1949—a decline of 70 per cent" (p. 129)—the volume of American imports did not show a corresponding increase. The reasons for the failure of imports to grow are investigated. The administrative red tape which hampers importers and exporters is discussed. The problem of American merchandisers selling foreign goods in the domestic market are explored. The difficulties confronting a European exporter who wishes to sell in the American market are spelled out. Then the question is raised as to which imports might under favorable conditions be increased. The case-method approach is employed, and a chapter is devoted to each of the following: agricultural imports, forest imports, mineral imports, petroleum imports, "luxury" imports, and tourism, shipping, and other services. If imports can be increased, someone will suffer. "Absolute or relative injury to some is an essential and inevitable result of the shift from a protected market to a free market" (p. 477). The next question is, accordingly, "Who would be hurt?" Here again the case method is employed, and peril-point decisions with reference to a number of commodities are reviewed, including among others hats, spring clothespins, blue-mold cheese, garlic, dried figs, and groundfish fillets. The answer appears to be that "(1) The exposed branches of agriculture will be injured. (2) Many of the older non-durable-goods industries will be injured" (p. 412). The author's final conclusion is: "Tariff reduction is not likely to be fully effective in stimulating trade so long as the escape clause promises to restore protection if imports threaten serious injury. Temporary tariff reduction is a dubious expedient. It is more important to avoid raising some tariffs than to reduce others" (p. 490).

Many cooks had a hand in the preparation of this broth. While it is undoubtedly too much to say that the soup was spoiled, it is likely that none of the chefs is completely satisfied with the finished product.

The book was written by Don D. Humphrey. It was published by the Twentieth Century Fund and was jointly sponsored by them and the National Planning Association. Calvin B. Hoover served as consultant and contributed an excellent chapter on "The European Exporter's Point of View." The final chapter, which, except for rare references to earlier portions of the book, might as appropriately have been appended to some other study of international economics, was prepared by a NPA committee, which is "solely responsible for the views and recommendations" set forth there (p. vii).

The fundamental issue which seems never to have been satisfactorily resolved concerns the audience for whom the book was written. A reading of the first chapter makes it clear that this was not intended as a book for the specialist in



international economics. The author feels, for example, that it is necessary to define the term "favorable balance of trade," and although the expression goes back in the literature at least as far as Sir James Steuart, the naive reader would be led to believe that it is related to a kind of stupidity largely confined to Americans "until after World War I" (p. 3). The term "international balance of payments" is also defined (p. 7) as is the term "portfolio investments" (p. 12). The statement is made without supporting evidence that the reciprocal agreements program was "designed to bring about international recovery by world-wide removal of trade barriers" (p. 5) although it was insisted by analysts writing at the time of the congressional debates on the program that neither free trade nor even low tariffs was among the objectives of the agreements. (See W. R. Allen, "International Trade Philosophy of Cordell Hull," *American Economic Review*, March, 1953, p. 113.) In a subsequent chapter it is stated explicitly that this is a book for the "general reader" (p. 38). But if this is intended for the non-specialist, the discussion at the end of the first chapter on the proper role of international investment for a creditor nation is likely to be confusing, and, in fact, neither here nor subsequently when the author properly observes that "foreign investment is a means of exporting without importing, but only so long as investments are continuously increased" (p. 452) is a careful distinction made between the creditor and the "mature" creditor nation which is standard in the better elementary textbooks in international economics.

Frequent references are made throughout the book to "classical" or "orthodox" international trade theory, and each time it is to suggest that certain modifications or amendments need to be made in the commonly accepted doctrine. This is probably a carry-over from the classroom, where the students may be expected to have some detailed knowledge of orthodox theory, but in a book for the general reader, if it be appropriate at all to attempt to refine the theory of international trade, it is important to spell out the accepted dogma in careful detail. Usually, as a matter of fact, the author has not come up with conclusions contrary to those usually accepted, but he has simply filled in details concerning which the general theory is silent. That the author is essentially orthodox, albeit perhaps somewhat reluctantly, is indicated in passages such as the following: "Trade is good—not because it will create additional jobs, but because it will make some jobs more productive. Trade is good—not primarily as a substitute for foreign aid, but because trade is mutually profitable" (p. 148). "The old argument that high-wage American labor cannot compete with cheap foreign labor is nonsense, . . . Our wages are high because productivity is high" (p. 460).

Considering the number of people who had to be pleased in the preparation of this volume, it turned out surprisingly well. Many of the thumbnail sketches of industries engaged in international trade are interesting and occasionally amusing. Much of the empirical detail may be useful to the specialist in international trade who prefers to have someone else do the fact grubbing for him. And Chapter 24, "Conclusions," is an excellent summary and index to the book which will make it easy for the reader who does not care to wade through the whole volume to locate those sections in which he is most interested.

*Agricultural and Mechanical College of Texas*

CLARK LEE ALLEN

*Capital Formation and Foreign Investment in Underdeveloped Areas.* By Charles Wolf, Jr., and Sidney C. Sufrin. Syracuse, N. Y.: Syracuse University Press, 1955. Pp. viii, 134. \$3.00.

Wolf and Sufrin have prepared an unusual volume. While not indicated by the title, their small book is not designed primarily as a study directly of economic development. Rather, it is "research on research" (p. 7)—past, current, and prospective—regarding development. They hope that "a partial inventory of research dealing with capital formation and foreign investment in underdeveloped areas" might provide "a basis for an analysis and evaluation of additional research needs and possibilities for applying the results or ideas" thus derived (p. 4).

Surely for most readers the heart of the book consists of a brief introductory Part I and a four-chapter Part II, together totaling some sixty-five pages.

Three topics are surveyed. The first is "entrepreneurship and the demand for capital." Reversing the usual emphasis, it is argued that frequently the crucial problem in underdeveloped areas is not a shortage of total resources available for capital formation, but a deficiency of proper demand for capital. Without attempting to measure the relevant magnitudes of potential capital supply, the authors contend that (a) capital formation may be increased by changing the pattern of investment; (b) the supply of savings may be more "flexible" than is generally supposed; and (c) there is a possibility of disinvesting accumulated "passive capital." Furthermore, there exist opportunities for profitable investment, in spite of problems associated with limited domestic markets. However, productive investment is inhibited by "the shortage of a *particular key or strategic resource: entrepreneurship*" (p. 21; authors' italics). Barriers to Schumpeterian entrepreneurship lie in adverse social values, especially when they become crystallized in inflexible institutions. If entrepreneurship is the key scarcity and if the largely inexplicable "islands of indigenous entrepreneurship" do not tend to diffuse through society, there may be implied an entrepreneurial role for government (pp. 22, 27-8, 43, 48, 52).

The second topic, "technological alternatives and the optimum use of capital," is concerned with achieving maximum productivity from any given stock of capital. The choice of production techniques is not necessarily confined to those now or previously used by the West: there is considerable scope for developing new innovations "to suit the socio-economic circumstances of the country" (p. 40). Input-output analysis and linear programming may assist in determining the optimum investment pattern. If there are sectors of the economy which "play a peculiarly generative role in sparking a process of overall growth" (p. 47), capital formation should be directed to them.

Relatively brief attention is given to "foreign investment and capital formation." It is feared that the climate for private foreign investment in underdeveloped areas is not highly encouraging and that most capital needs must be met, as has been the case historically, from domestic sources. But foreign investment may trigger indigenous capital formation—especially direct investment, which includes management.

All of this is quite well done, although the scope is admittedly uncomprehensive, and the treatment is brief. Especially in light of the basic nature of the re-

port, a more complete and systematic citation of the literature would have been appropriate. The major value of the sections on "program possibilities and research needs" following each of the three main chapters is simply to reiterate certain key elements in the general analyses.

An annotated bibliography (pp. 69-113) constitutes a major portion of the volume. It is well organized and represents a rather ambitious effort. However, only selected titles are included, and the marginal utility associated with additional entries would still be high.

The authors have performed a useful task in summarizing and evaluating large portions of the completed and required work in the nebulously delimited area of "development." The book is a bit encumbered, in both organization and content, by undue emphasis on procedural matters concerning the preparation of the book and the pattern of desired research. But perhaps such product differentiation is a condition of publication.

University of California

WILLIAM R. ALLEN

*Promoting Economic Development: The United States and Southern Asia.* By Edward S. Mason. Claremont, Calif.: Claremont College, 1955. Pp. 83. \$2.75.

Dean Mason's interesting eighty-three page study is a revision of a series of lectures given during 1954 before non-professional audiences in California. Because of the character of his listeners, the book is necessarily non-technical in nature and is suited for the layman as well as for the professional reader.

The author's stated purpose is to raise questions concerning proper United States policy toward underdeveloped countries rather than to present a case for any particular policy. However, the reader will find more than a passing interest in particular cases, since the bulk of the book is devoted to a careful development of the author's own policy preferences.

He begins his discussion with an examination of the reasons for American interest in the development of backward countries. After minimizing the importance of the usual humanitarian and economic motives as public policy objectives, he rests his case for continued interest in these areas largely on security reasons. To him the communist threat is both military and politicoeconomic and must be resisted by economic development as well as by military strength. He is moderately optimistic about the possibilities of raising per capita incomes in these areas if inordinate increases in population can be prevented, but suggests that such growth probably will not be possible without substantial external economic assistance. While recognizing full well that any development resulting from such assistance may not alone prevent the spread of Communism, he concludes that such development is a necessary step in the battle and recommends substantial United States foreign aid. The final chapter, which is based upon the author's first-hand experience as a consultant to the government of Pakistan, is a case study of the development possibilities of that country.

This is a competent study that deserves little adverse comment. Perhaps it might well be mentioned, however, that the author devotes less space to policy alternatives than seems desirable in a book of this type. This is a relatively minor

point, however, and does not detract seriously from his skillful presentation of the essence of a very vital problem in a few short pages. Of particular interest to the economist is his original analysis of the development problems confronting Pakistan.

*University of Tennessee*

ROBERT P. BLACK

*Economic Growth: Brazil, India, Japan.* Edited by Simon Kuznets, Wilbert E. Moore, and Joseph J. Spengler. Durham, N. C.: Duke University Press, 1955. Pp. xi, 613. \$12.50.

The eighteen papers in this volume grew out of a conference sponsored in 1952 by the Committee on Economic Growth of the Social Science Research Council. How broadly the subject was conceived by the planners of the venture is suggested by the division of the papers into three groups: economic trends, demographic factors, and government and social structure. One or more papers on each of the three countries is found in each group. This provides some comparative treatment of the countries, though no paper specifically attempts a comparative analysis. There are six papers on economic trends, four on population, and eight on the role of government and social structure.

The countries chosen for study provide great diversity of economic conditions and experience. They have little in common except that seventy-five years ago all had low per capita incomes. In population, resource endowment, industrialization, political experience, climate, and the like, at least one of the three differs significantly from one or both of the other two countries. Consequently, theories of economic growth which proved valid for these three countries might have some claim to generality. None of the papers in this volume presents such a theory, however, and the diversity of the countries serves only to illustrate the complexity of the common problem of economic growth.

The authors vary in their approaches. Some offer a survey of the statistical data pertinent to a particular topic, such as national income or population. Here the most striking result is the paucity of data. For example, Daniel Thorner was unable to determine whether per capita income has risen or fallen in India in the past century. Though population statistics are better, the trend in birth rates, infant mortality and other crucial measures can often not be established. Among the three countries, the data appear to be best for Japan and worst for Brazil. Faced with such a lack of basic data, one may sympathize with Kuznets' plea for research to clarify the basic trends in economic activity.

Some authors present a description of historical events, or of the existing state of society. A few of these simply summarize facts that are well-known and available in standard works; others offer a critical survey of the literature or provide useful new insights and interpretations. Lockwood has two outstanding papers on Japan that fall into this latter category. (As he points out, they are substantially the same as two chapters in his book on Japan.)

A third and smaller group of authors attempt to bring a theory, or some form of analytical framework, to the study of an aspect of a country's experience. Reubens' essay on Japanese use of foreign capital, and Levy's provocative analy-

sis of the influence of social structure on the course of development in China and Japan, are worthy of especial mention.

No comprehensive theory of growth is presented in this book, as has been mentioned. Few new statistical or factual findings are reported, but this was not the purpose of the book. No unified analysis or comparative studies of Brazil, India, and Japan are to be found here. The book is, indeed, typical of the vast and little explored field of economic growth. These papers, which branch off in all directions, emphasize that this is not a clearly defined area of discourse.

The volume is useful as an illustration of the type of work being attempted in the field of economic growth, and as an indication of the primitive state of knowledge about the dynamics of growth in these particular countries. In some cases it provides a handy summary of the literature where the earlier work is not readily available. It suggests, however, that the search for economic trends may have to begin in the present rather than in the past, for in many cases no statistical "finds" capable of yielding reliable data for past decades may reasonably be expected.

*University of North Carolina*

JAMES C. INGRAM

*Readings in Fiscal Policy.* Edited by Arthur Smithies and J. Keith Butters. Homewood, Ill.: Richard D. Irwin, 1955. Pp. x, 596. \$5.00.

This is the seventh volume of readings sponsored by the American Economic Association. Since some aspects of fiscal policy were covered in earlier volumes on business cycles and monetary theory, the editors of the present volume state that their primary objective is to augment and to supplement the previous coverage. Also, to avoid unnecessary duplication, significant articles readily available in other collections of readings are omitted. Consequently the thirty-four articles contained in this collection may not be interpreted as an exhaustive compilation of the most important contributions in the field.

As a subsidiary objective the editors have sought to show how fiscal policy doctrine has evolved over the past generation. This may serve to explain the inclusion of such articles as "The Economics of Public Works" by Sumner H. Slichter, "Federal Depression Financing and Its Consequences" by Harley L. Lutz and "The Federal Budget: Economic Consequences of Deficit Financing" by B. F. Haley.

In a brief review it is possible to do little more than indicate the general organization of the book, to mention a few outstanding contributions, and to attempt a broad assessment of the volume as a whole. The readings are divided into seven parts or categories. The first is a one article introduction on the balanced budget by Jesse Burkhead. Professor Burkhead provides a convincing relationship between economic theory on the subject and American folklore. The second section contains five articles on fiscal policy in recovery. "An Open Letter" by J. M. Keynes and "Fiscal Policy in the Business Cycle" by Gunnar Myrdal are outstanding. Part three on fiscal policy and inflation contains six papers. Of these the ones by William J. Fellner and Keynes deserve mention. The sixteen papers in part four are supposed to deal with fiscal policy and the national debt, mon-



etary policy being included. Yet some of the papers appear to escape even this broad category. This reader was impressed most by the contributions of John H. Williams and Arthur Smithies. None of the papers in part six on burdens of the budget and debt are noteworthy, although the one by A. P. Lerner probably warrants inclusion on the basis of uniqueness. Part seven on economic growth contains two papers, one by J. A. Schumpeter and one by Alvin H. Hansen. Both are outstanding. The volume concludes with an excellent bibliography of articles on fiscal policy prepared by Wilbur A. Steger.

Undoubtedly the collection is a worthwhile addition to current readings in fiscal policy. Complete agreement among specialists concerning individual selections is hardly to be expected. Nevertheless, a sufficient number of significant contributions have been inserted to justify tolerance towards the others. The collection might be compared to the little girl with the curl in the middle of her forehead. Almost always she is very, very good.

*University of Maryland*

HENRY GRAYSON

*Federal Debt-Management Policies, 1865-1879.* By Robert T. Patterson. Durham, N. C.: Duke University Press, 1954. Pp. xi, 244. \$4.50.

Through an examination of the handling of the United States government debt during a period when it was less than one-tenth of its present size and when the economic and social structure was much less complex, the author has sought to derive some lessons for our own day. In the course of his study he has given us a rather detailed survey of the shifts in policies followed by eleven different Secretaries of the Treasury under five different Presidents. In the course of the text and through his extensive bibliography, the author has shown an intimate knowledge of the literature dealing with this problem.

Against the familiar backdrop of the country's financial unpreparedness for war, he examines the needs that forced us into the exigencies of short term borrowing and the greenback experiment. At the end of the war there was a range of public attitudes toward the debt all the way from those who regarded it as a burden to be reduced quickly, through those who wished to make a virtue of it by making it permanent with perhaps only gradual reduction, to those who called for its conversion into fiat money. Secretary McCulloch pursued the conservative policy aimed at restoring the value of the currency and enhancing the public credit by contracting the greenbacks, reducing the over-all debt, and converting the floating and short term obligations. In the achievement of these goals, the policy was carried to the opposite extreme by issuing long term, non-callable bonds, which the author refers to in this work (p. 83ff) and elsewhere, as "a major error in debt policy." He attributes this error to the confusion and contradictions of the post-war period, among them the conflicting objectives of tax reduction and debt reduction, together with the illusion of the sinking fund mechanism.

Throughout the book the author endeavors to analyze the shifting events in terms of their impact on the broader objectives of fiscal policy, such as the influence upon the money supply, production, prices, capital formation, invest-

ment, and the distribution of income. If at times this analysis seems somewhat forced, certainly the fault lies not in the author's efforts but rather in the original thesis with respect to the comparability of this period with World War II and its aftermath. To this reviewer the author's own concluding paragraph seems to support this view. It is as follows:

With the resumption of specie payments and the completion of the refunding program, too (sic) great over-all objectives of debt management were achieved: the currency was brought to a parity with gold, and the public credit was fully re-established (p. 220).

*University of Florida*

C. H. DONOVAN

*The Price System and Resource Allocation.* By Richard H. Leftwich. New York: Rinehart & Company, 1955. Pp. x, 372.

Two primary objectives are stated in the preface of this book. (1) It is intended as a textbook for courses in price theory at the junior-senior university level, and (2) it is designed to provide a review of the major principles of price theory and resource allocation for graduate students in economics. Although both of these objectives have been accomplished in an effective manner, it is believed that the book will be more valuable in satisfying the second objective than the first.

This work is an excellent restatement of orthodox price theory. The explanations in the book are concise and clearly stated. The author has wasted very few words in writing this book. He has not been guilty of the somewhat overworked practice of overcomplicating the obvious, but rather has chosen to present his analysis in as simple a manner as possible with adequate illustrations which are explained clearly in the text.

In his own words the author has been "selective rather than exhaustive" in his coverage. Little attention has been devoted to refinements in the theory or to application of the theory to policy. This approach gives the book some of its strength but also subjects it to certain limitations. In choosing to do a complete job in restating price and distribution theory, the author has avoided many difficulties in choice of material. On the other hand, as a book devoted to an analysis of skeleton price theory, without refinements or application to policy, the book finds itself in an in-between position. As stated by the author, the book would not be suitable for a first course in economics for it lacks descriptive material on the economic system. As a book designed for junior-senior courses in economics (with a prerequisite of six hours in basic economics), it lacks contribution beyond what should have been learned in those basic courses. Essentially, it would review for students of this level what they had already learned in a principles course, but would add very little to their knowledge. As a review of price and distribution theory for graduate students who feel that they are rusty in economic principles, this book would be excellent.

As is usually the case in orthodox economics, the portion of this book devoted to price theory under varying marketing conditions is much stronger than the portion devoted to distribution theory and to an explanation of equilibrium conditions. However, the author has reviewed and summarized the existing theory

on these points in a most able manner. His chapter XVII on equilibrium is as simplified a statement of this concept as can be found in 15 pages anywhere known to this reviewer.

*Louisiana State University*

W. H. BAUGHN

*Income of the American People.* By Herman P. Miller. New York: John Wiley & Sons, 1955. Pp. xvi, 206. \$5.50.

Herman Miller has written "a book about people and income." He has written it from the vantage point of eight years' experience in collecting and analyzing income data for the Bureau of the Census. Accordingly, this second work in the Census Monograph Series sponsored by the Social Science Research Council represents a real contribution in the field of income research. Not only specialists in income work but also teachers of elementary economics, consumption economics, and labor courses will find the book interesting and useful.

Miller orients his book around three questions: What determines an individual's income? What determines a family's income? Why has income become more equally distributed since the 1930's? In order to answer these questions, Miller draws upon the wealth of income information available in the 1940 and 1950 censuses including some previously unpublished tabulations together with supplementary material from smaller samples and other sources. Necessarily the answers he gives cannot be pursued in all cases to their ultimate conclusion but instead focus upon the personal attributes of sex, color, education, age, occupation, residence, and the like that form the basic variables of population censuses. He does, however, go beyond these variables wherever possible, and draws from supplementary sources to suggest important factors that ultimately determine individual and family incomes. In instances, he quotes from studies of worker mobility, color problems in industry, wage rates, and others.

Miller clearly puts his vote on occupation as the most important factor determining an individual's income. His explanations of color and residence influences on income heavily stress occupational characteristics associated with these attributes. Accordingly, the main emphasis of the book is upon analyzing the relation between occupations and incomes. He finds that for most occupations there is an essentially normal distribution of individuals by size of their earnings. Only for occupations with uncertain receipts (entrepreneurs and farmers) or with irregular employment (service workers) is there any great degree of skewness in the distributions. This means that the total income distribution is asymmetric largely because of differences in incomes for the occupations that make up the total distribution. Miller leaves for others the problem of explaining differences in income levels between occupations, but he notes that evidence does exist of occupational mobility between and within generations, which at least suggests that occupational income differences do not arise completely from monopoly elements.

After dealing with individuals, Miller puts the pieces together and deals with the family. He devotes some time to the way that income affects family formation and how the working wife's efforts result in a change in the total distribution. Particularly interesting are some data he presents on income of the aged, sug-

gesting, contrary to popular belief, that most older workers retire from the labor force voluntarily and not because they lack opportunities to work.

Finally, the author examines some causes of the decreased inequality in income distribution since the 1930's insofar as the census data permit. He finds that changes in the occupational structure, increases in the proportion of full-time workers, narrowing of differentials within and between occupations, increased number of supplementary earners, and the improved position of farmers have been important causes of the decline in inequality.

Of particular interest to readers of this Journal are Miller's thoughts on the difference in incomes between South and non-South and his treatment of the incomes of non-whites. He finds that his data do not support D. Gale Johnson's thesis that incomes of whites residing in communities of similar size are approximately equal. Incomes of male whites of similar ages are lower in Southern cities than in cities of comparable size in the North Central region and in the West. In large Southern cities, median incomes of white males are above those of Northeastern major cities. Color differences in incomes he traces mainly to exclusion from many occupations, an exclusion that is more complete in small cities than large cities and that is apparently not related to educational differences between color groups.

It is impossible in the space of this review to adequately treat many of Miller's major ideas. The book is easy to read and relies on simple tabular presentation rather than elaborate statistical treatment to make the points. The author has wisely chosen to cover a variety of topics in this field rather than beating one topic to death as is so often the case. Nevertheless, definitions and explanations of the census income data in three appendixes provide all the technical information necessary to judge the adequacy of the findings.

*Federal Reserve Bank of Atlanta*

THOMAS R. ATKINSON

*Distribution's Place in the American Economy Since 1869.* By Harold Barger. Princeton, N. J.: Princeton University Press, 1955. Pp. xviii, 222. \$4.50.

This book is a statistical exploration of certain historical developments in the areas of wholesaling and retailing. It is a research study in the general series, and authored by a member of the research staff of the National Bureau of Economic Research.

Part I, Trends in Employment and Output, contains three chapters. They are: chapter 1, Distribution's Growing Share of the Labor Force; chapter 2, How Fast Did Output Expand?; and chapter 3, Productivity and Its Measurement. Part II, The History of Distribution Cost, consists of these: chapter 4, Measures of the Cost of Distribution; chapter 5, The Changing Channels of Distribution; chapter 6, Trends in Margins; and chapter 7, Measures of Spread by Kind of Outlet. These seven chapters occupy ninety-five pages.

The appendixes and the index require one hundred and twenty-two pages. The four appendixes are entitled: A, Employment, Earnings, and Labor Income in Distribution and Commodity Production; B, Procedures Used in Estimating Value Added by Distribution; C, Sources of Margin Data; and D, Bibliography of Periodicals in Retail and Wholesale Trade to 1919.

The author's interest is in examining a single service industry, distribution. He is curious about some of the changes which have occurred in this industry since 1869, and he wants to contrast trends as between commodity production and commodity distribution. Commodity production is taken to include manufacturing; mining; agriculture, forestry, and fishing. Commodity distribution is defined as wholesale and retail trade. Emphasis throughout is on quantitative measurement.

Here are the major findings. (1) From 1869 to 1950 the percentage of the labor force in distribution increased steadily; between 1930 and 1950 the proportion in distribution rose from 12 per cent to about 17 per cent. During the longer period, the proportion of the labor force in production declined; for the shorter period, 1930-1950, this figure fell from 50 per cent to 40 per cent. (2) Between 1869 and 1950 output per man-hour in distribution rose, though much more slowly than corresponding output in production. Between 1929 and 1949 distribution output rose by 20 per cent; production output rose by 67 per cent. The small number of technological innovations in distribution is one explanation. (3) Distribution's share of the retail sales dollar increased definitely but slowly from 1869 to 1919; it has been decidedly stable since 1920. Services offered by retailers to their customers are probably greater than in 1869, but the change is small.

Painstaking research went into this book. Thirty-seven tables and four charts support the text. Concise summaries are included at several points. The book is clearly a contribution to our knowledge about distribution.

The findings are not news to marketing men. Furthermore, few of those individuals have more than a slender interest in the 1869-1920 period. They will, of course, welcome Barger's affiliation with the group which favors the term "value added by distribution" over "distribution costs." They will deplore his dealing with one phase of the distribution industry rather than with the function of distribution, or, even better, with the function of marketing.

University of North Carolina

C. A. KIRKPATRICK

*The Redistribution of Income in Postwar Britain: A Study of the Effects of the Central Government Fiscal Program in 1948-1949.* By Allan M. Cartter. New Haven, Conn.: Yale University Press, 1955. Pp. xi, 232. \$5.00.

This volume, the third of *Yale Studies in Economics*, is a careful analysis of the effect of the Labour Government's taxation and expenditure program for the fiscal year April 1948 to April 1949 on the re-distribution of income in Great Britain. Part 1 includes a description of the theoretical problems involved in the statistical measures, and Part 2, half the book, is "an outside technical appendix" which explains in detail the use and combination of source materials and the allocation of particular taxes and expenditures. The results of the study of 1948-1949 are compared with Tibor Barna's study of the pre-war year 1937. Also, after adjustments for differences in functions between the central governments in Great Britain and the U.S.A., a comparison is made between the British figures and those of John H. Adler's study of the American economy for the years 1938-39 and 1946-47.



The author set out to answer a series of questions. "Who really gained and lost by the re-distribution of income in Great Britain? Did more or fewer persons gain than in the years immediately before the war, and how much? Did the degree of distribution change markedly in relation to the level of national income? Was more or less of tax revenue being re-distributed? How much of this was due to the Labour government's efforts? How have the middle classes fared?"

In the quest for answers the author uses ingenious and painstaking methods of combining data from various sources. He also has an admirable and disarming habit of stating clearly his assumptions and his reasons for making them. Since his aim was to determine the direct income effects of the government program, and in the belief that the program was intended to re-distribute income from the relatively high to the relatively low levels, his observations were made and results set up in terms of groups classified by income levels. And since he is concerned with policy and its effects, he examines the receipts and expenditures legislated in a single fiscal year, rather than those actually received and disbursed. Government expenditures are classified into divisible and indivisible. The former, about 40 per cent, are transfers without the performance of reciprocal services and other expenditures which can be allocated to income groups with reasonable accuracy: the latter include such items as expenditures on national defense and the system of justice. Allocation of the indivisible items is made on three different assumptions and "the reader may choose which of these, if any, he will accept," although for comparative purposes the author uses the "median" one that benefits are proportional to net private income.

The major conclusions of the study are not surprising. As a result of the overall program of taxation and spending in 1948-49, there was substantial redistribution of income, estimated as amounting to 13.1 per cent of the total national income. This compared with 8.8 per cent of national income re-distributed in 1937. In 1948-49, slightly less per £ of tax revenue was re-distributed than in 1937, because a number of the post-war welfare programs, such as the National Health Service and Family Allowances, provided approximately equal per capita rather than strictly distributive benefits. The break-even point, where the average taxpayer contributed as much as he received from the government, came in the income range £550 to £650. "Nearly all wage-earners registered net gains, while all well-to-do persons and a majority of those in the middle £500-2,000 group were net losers in 1948-49." All three major parties in Britain have supported the welfare programs and there has been no clear-cut political issue on the extent of re-distribution.

Reference has already been made to the care and ingenuity expended on materials for the study. Nevertheless this reviewer is impelled to question some of the conclusions in the summary of Chapter 10 and one matter of presentation. While the text context makes the point clear, should not Table 1 of the book state that the "number" of persons is in thousands and not in units? In the summary, can the statement that "many people have gained while a somewhat smaller number have lost" be reconciled with the statistical conclusion that

"about 80 per cent of the population was on the gaining end?" The anomalies in the summary statement are puzzling since the bulk of the work shows meticulous care. This care, in addition to perspective, is shown in the author's statements about the limitations of statistical analysis and the questions he raises show perceptive insight into the relation between economic, psychological and political issues. As a scholar he has broken new ground in his use of data and one hopes he will continue to cultivate it by producing other comparable studies. This is particularly true since he writes with ease and grace, and while answering some questions, raises others which anyone interested in contemporary Britain would be glad to have answered.

*Sweet Briar College*

GLADYS BOONE

*Economics and Liberalism: Collected Papers.* By Overton H. Taylor. Cambridge, Mass.: Harvard University Press, 1955. Pp. 321. \$5.00.

This volume contains reprints of twelve essays which were published by Professor Taylor at somewhat irregular intervals during the period 1927-1952. In addition, the author has written an introduction in which he comments briefly on the contents of the essays and explains the circumstances under which each was written. As the blurb on the jacket correctly observes, these writings virtually constitute an intellectual biography of a man who has more or less consistently remained a "liberal" in the classical meaning of the word.

Taylor's work reflects a singular lack of interest in the intricacies of what is frequently called formal or "pure" economic theory. He candidly states his conviction that economic theory is not an "isolated, self-sufficient, independent inquiry," but is instead only one of many integral parts of a social philosophy. And it is with the other parts of a social philosophy, *e.g.*, political theory, philosophy, psychology, etc. that most of his writings are concerned. Or perhaps more correctly, Taylor is primarily interested in attempting to understand and bring into sharper focus the interrelationships of all the disciplines which have contributed to the development of the social philosophy called liberalism. For example, two of the essays deal with the relationship of natural-law theory to the classical pattern of economic ideas, and a third analyzes the impact of broad philosophic systems on the formation of political economy. Another is devoted to the relationship of economics and politics. Thus one may readily understand why Taylor classifies himself as a "philosophical political economist," for his writings cover a much broader area than that which is traditionally assigned to "economists."

One of the more interesting features of these essays is the author's insistence that liberalism should not be identified with any "single, fixed, precise ideal and body of doctrine." He defines liberalism as the "philosophy . . . and approximate practice of the free way of life in all its aspects: economic, social, cultural and ethical, juridical, political" (p. 296). According to Taylor, the meaning of liberal capitalism will vary from time to time as the economic, social and political requirements of nations change. However, he thinks the basic institutional fabric of liberalism provides the most desirable framework for attaining the constantly

changing goals of a dynamic society. As an example in the broad area of policy problems, he argues that our economic system can accommodate itself to the "Keynesian" kind of relatively full-employment stabilization without subverting the essential principles of liberal capitalism. To be sure, he recognizes that such a stabilization policy contains inherent dangers, but the essential point is his firm conviction that this, and other important social and economic problems, can be resolved within the liberal framework.

Expressions of personal opinions and value judgments are to be expected in any body of writing which purports to be both an analysis and defense of liberalism. However, Taylor rarely, if ever, writes as though he were trying to dispense truth with a capital "T." On the contrary, his intellectual modesty stands in sharp contrast to the more common type of dogmatic certitude one finds in many "defenses" of economic liberalism. In any event, those who are interested in the past, as well as the future, problems of liberalism will find this collection of essays both stimulating and rewarding.

*Agricultural and Mechanical College of Texas*

ALFRED F. CHALK

*The Economics of Group Banking.* By Palmer T. Hogenson. Washington, D. C.: Public Affairs Press, 1955. Pp. vii, 199. \$5.00.

Dr. P. T. Hogenson in his *Economics of Group Banking* has written a study of group banking in the United States. Group banking as distinguished from multiple office banking refers to organizations in which more than one bank is controlled by a holding company or voting trust. As of 1952, the relative importance of group banking can be measured by statistics which show that in 3 states group banks controlled 50 per cent or more of all deposits and in 13 additional states group banks controlled 20 per cent or more of all bank deposits. The study covers all relevant financial and governmental source material and this material treated on a geographic basis. The author has treated the Central Northwest area of the United States with special emphasis and has made detailed analyses of the group banks operating in the area.

According to the findings, consolidation of banks into a regional system does not heighten necessarily market control since banking (in one bank localities) is a local monopoly. The study shows that group banking in most bases did not develop from attempts of entrepreneurs to exploit larger scale enterprise but rather from attempts to secure promotional and speculative profits. Other than promotional reasons, Hogenson states that growing industrial needs for credit required that the capital structure of banks be enlarged; for institutional reasons, this capital growth could not be provided locally. As result, security promotions in outside markets provided the financial solution for this problem. However, the most interesting reason cited by Hogenson explaining the growth of group banking is that the institution was an evasion of state laws prohibiting branch banking. The author claims that "group banking is an intermediate step between independent unit banking and branch banking on a state wide or regional basis." As evidence for this conclusion, Hogenson points out that when state banking

laws were revised as a result of the trend towards group banking, wherever revisions permitted group banks became branch banks.

The book is recommended for those who seek more information than can be found in the average money and banking text. However, despite the title, as an analysis of the *economics* of large scale banking the study leaves much to be desired. For a more thorough analysis of market control in banking, the reader is advised to read D. A. Alhadeff's *Monopoly and Competition in Banking*, Berkeley, 1954 (reviewed in this *Journal*, April 1955).

University of Georgia

ROBERT R. DINCE, JR.

*The Life of Lord Nuffield: A Study in Enterprise and Benevolence.* By P. W. S. Andrews and Elizabeth Brunner. Oxford: Basil Blackwell, 1955. Pp. xvi, 356. 25/- (\$3.50).

Viscount Nuffield of Nuffield was born William Richard Morris. Bicycle maker, garageman (the M. G. car takes its name from Morris Garages), and automobile manufacturer, Morris amassed the fortune which put the Nuffield label on British philanthropy for a quarter of a century. Oxford's Nuffield College is one of numerous educational, medical, and research institutions sharing in gifts totalling over £25 million.

Conceived as "an act of piety which would enable Nuffield College to have a record of that remarkable enterprise, and of the man who made it and from its fruits endowed the college," this "first 'official' life of Lord Nuffield" leaves unanswered many questions that would interest the entrepreneurial historian. While the authors have kept their promise to avoid "excessive laudation," one looks in vain for a forthright admission that Nuffield had any shortcomings. Severer critics might put a different interpretation on ruthless price-cutting, the slashing of dealers' commissions, and hard bargaining with suppliers, which were some of the tactics employed by Morris to capture almost half the British motor market. The creator of the "Ford with an Oxford education" fulfilled his promise to "bring motoring within the reach of many apart from the rich," but in England as in America large profits depend upon something more than the urge to please the consumer. In the 1920's eighty-one makes of cars disappeared in the "inevitable shake-out" of competing firms. In 1952 the formation of the British Motor Corporation by the merger of the Morris and Austin interests created the third largest motor manufacturing company in the world.

Duke University

ROBERT S. SMITH

*Free Trade and Protection in the Netherlands, 1816-30: A Study of the First Benelux.* By H. R. C. Wright. New York: Cambridge University Press, 1955. Pp. x, 251. \$6.00.

The economic integration of the Netherlands and Belgium-Luxembourg into Benelux has achieved impressive progress in restoring the unity of the Low Countries that was broken in 1830. That union was the political creature of the Congress of Vienna. It was intended, on the one hand, as a dam against French

expansion, and it was believed, on the other hand, that by uniting industrial and agricultural Belgium with the commercial, maritime and colonial Netherlands a harmonious economic structure would be established. But the political union was shortlived, having been destroyed after fourteen years duration by the Belgian Revolution. What was the cause?

Dr. H. R. C. Wright attempts to answer the question in the book under review. In this fine, scholarly work, described as "a study of the first Benelux," he has reconstructed the economic history of the Low Countries on the basis of Dutch and other documentary materials. From the amassed evidence he concludes that though antagonism was inherent in the different economic structures and policies of the two parts of the kingdom—the traditionally free-trade Dutch provinces whose economy "centered on the international staple market in Holland" and "the Belgian provinces, where French rule . . . had stimulated capitalist undertakings," which demanded protection, these were not responsible for the violent dissolution of the union. King William I's economic policy—here discussed in detail probably for the first time in English—that aimed to integrate and strengthen the provinces through achieving a common prosperity for them and thus reconcile their conflicting interests had by 1830 achieved much success. It was political friction between the two peoples, especially Belgian national aspirations, that disrupted the union.

Against a background of the different economic and fiscal traditions of the two provinces provided in Chapters I-V, the author devotes by far the larger part of the book (Chapters VI-XIII) to William I's economic measures for harmonizing the conflicting Dutch and Belgian interests and to the causes of their failure. He describes the king's attempts in 1816 and again in 1821 to find a fiscal system acceptable to both peoples, as well as the positive measures which the government adopted for the encouragement of industry, commerce and agriculture. He also shows how the Dutch East Indies colonies were made useful to both Dutch and Belgian interests. However, while the monarch gradually shaped a coherent economic policy, he "did not succeed in giving his kingdom any real economic cohesion. The northern and southern provinces never formed a single national market, and their currencies remained separate for all ordinary purposes. The Dutch showed little interest in private investment in Belgium. In fact, the countries developed separately within the framework created by William I" (p. 223).

Despite this development, "It has been suggested that William I's policy contributed to the Belgian Revolution by its very success: that the prosperity of Belgium made her more confident of her national destiny and less willing to remain a junior partner to the stagnant Dutch" (p. 222). But Wright points out the very interesting fact that "The main revolutionary element in 1830 in Belgium was the miscellaneous urban middle class, and especially the lawyers and other professional men," as well as the discontented "educated young men for whom there were no longer such attractive opportunities as under Napoleon" and who resented the presence of Dutch officials in Belgium, and not, significantly enough, the predominant Belgian economic interests. These gained most



by Dutch-Belgian union and were Orangist in sentiment. And, the revolutionary organizations of the Belgian middle class "used the workingclass movement" to achieve Belgian independence.

One feels in reading the book that the author believes, though he never says so explicitly, that the union of the Low Countries would have persisted but for Belgian nationalism. It therefore is to be hoped that the "second" Benelux which has achieved considerable results without political unity will prove of more permanent duration.

Dr. Wright's book should be of interest not only to students of the economic history of the Netherlands but also of Europe.

*Washington, D. C.*

ARTHUR LEON HORNIKER

*Selected Antitrust Cases.* By Irwin M. Stelzer. Chicago, Ill.: Richard D. Irwin, 1955. Pp. x, 210. \$3.50.

This book consists of a compilation of landmark judicial decisions in the field of antitrust law. With the exception of a brief introductory note by the editor at the beginning of each chapter, the entire book contains a reproduction of the significant portions of 35 court opinions which have substantially affected the application of the antitrust laws to American business.

This book should provide material assistance in the teaching of various types of courses which examine governmental policy toward the regulation of business activity. There may be some who prefer to utilize the case method as a basic approach, and who desire to adopt this book as the fundamental material for the structure of a course. It is probable, however, that this book will best serve its purpose as supplementary reading in one accessible package, to provide students with the actual flavor and reasoning of those court decisions to which reference must be made in the area of governmental regulation of business.

The cases have been selected and grouped by the editor with obvious care and excellent results. The omission of portions of the cases, in order to avoid undue length and unnecessary legal technicality, has been accomplished without disturbing the even flow of the legal reasoning and without depriving the student of the essential heart of each case. When 35 cases are selected for presentation as the outstanding decisions in the antitrust field, there will always be room for legitimate differences of opinion concerning the particular cases which merit inclusion within such a distinguished group. However, the basic landmarks are all present in this book and will furnish to the student a genuine picture of the many paths which have been traveled by our antitrust laws.

*University of North Carolina*

GERALD A. BARRETT

## NOTES

### ACKNOWLEDGEMENTS

The Southern Economic Association herewith gratefully acknowledges financial contributions made to *The Southern Economic Journal* by the following institutional members:

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### ANNOUNCEMENT

The twenty-sixth annual conference of the Southern Economic Association will be held on November 16 and 17, 1956 at the Sir Walter Hotel, Raleigh, North Carolina.

President Edward H. Anderson has appointed the Nominating Committee for the 1957 officers of the Southern Economic Association. It consists of William H. Nicholls, Vanderbilt University, Chairman; John B. McFerrin, University of Florida; and William H. Baughn, Louisiana State University. Members are invited to suggest names to this committee.

HOWARD R. SMITH  
*Secretary*

## THE SOUTHERN ECONOMIC JOURNAL

Receipts and Expenditures for the Fiscal Year Ending October 31, 1955		
Cash Balance, November 1, 1954.....		\$ 4,697.97
RECEIPTS		
University of North Carolina		
Balance Annual Grant for 1954-1955.....	\$1,000.00	
Advance on Annual Grant for 1955-1956.....	800.00	
Total.....		\$1,800.00
Southern Economic Association		
Annual Membership dues.....	\$2,220.00	
Institutional Membership dues.....	600.00	
Contributing Membership dues.....	150.00	
Student Membership dues.....	22.50	
Total.....		\$2,992.50
Office of Managing Editor		
Subscriptions to Journal.....	\$2,488.37	
Miscellaneous Sales of Journal.....	398.42	
Advertising.....	1,634.99	
Total.....		\$4,521.78
Total Income.....		\$ 9,314.28
Total Cash Balance and Income.....		<u>\$14,012.25</u>

EXPENDITURES		
Printing the Journal.....	\$6,731.89	
General Expense.....	68.56	
Other Printing.....	93.65	
Postage.....	220.38	
Telephone, Telegraph and Supplies.....	164.91	
Salary.....	2,383.33	
Travel.....	126.08	
Advertising.....	40.00	
Refunds.....	5.50	
Total Expense.....		\$9,834.30
Balance, October 31, 1955.....		<u>\$4,177.95</u>
		<u>\$14,012.25</u>
Balance Represents		
UNC Advance on 1955-1956 Annual Grant.....	\$ 800.00	
Actual Balance.....	3,377.95	
		<u>\$4,177.96</u>

G. T. SCHWENNING  
Managing Editor

## SOUTHERN ECONOMIC ASSOCIATION

*Receipts and Expenditures*

CASH ON HAND, NOVEMBER 1, 1954 .....		\$ 839.44
RECEIPTS, Fiscal Year Ending October 31, 1955:		
Annual Memberships .....	\$2,780.00	
Student Memberships .....	30.00	
Institutional Memberships .....	600.00	
Contributing Memberships .....	150.00	
Savings and Loan Dividends .....	17.23	
Miscellaneous .....	10.10	3,587.33
		<u>4,426.77</u>
EXPENDITURES, Fiscal Year Ending October 31, 1955:		
Association Expenses:		
Postage .....	\$ 39.47	
Supplies .....	73.08	
Printing .....	203.93	
Travel .....	81.30	
Miscellaneous .....	135.80	533.58
Investment account:		
Dividend Deposited .....	17.23	
Southern Economic Journal .....	2,992.50	3,543.31
		<u>883.46</u>
CASH ON HAND, October 31, 1955 .....		<u>4,426.77</u>

*Investment Account*

BALANCE, November 1, 1954 .....	570.25
DIVIDEND Deposited .....	17.23
	<u>587.48</u>
BALANCE, October 31, 1955 .....	<u>587.48</u>

*Fund Balances*

CHECKING ACCOUNT, National Bank of Athens, Athens, Georgia .....	883.46
INVESTMENT ACCOUNT, Athens Federal Savings and Loan Association, Athens, Georgia .....	587.48
	<u>1,470.94</u>
TOTAL FUNDS, October 31, 1955 .....	<u>1,470.94</u>

*Athens, Georgia*  
*November 1, 1955*

HOWARD R. SMITH  
*Treasurer*

## PERSONNEL NOTES

Hayward Anderson, formerly at Pearl River Junior College, is now instructor of accounting at Mississippi Southern College.

William Cathcart, of Kansas State College, has accepted an appointment as instructor in agricultural economics at Oklahoma A. and M. College.

Omar D. Craig is now instructor in accounting at the University of Mississippi.

J. H. Dalton, assistant professor, joined the economics faculty of the Uni-

versity of Maryland last September. He was formerly at the University of California.

Harold R. Demerest has been appointed associate professor of institutional management at Mississippi State College.

H. E. Dennison has been appointed lecturer in business administration at the School of Business Administration, Emory University.

Mary Dunstan has resigned as assistant professor of marketing at Mississippi Southern College.

H. C. Edgeworth is now assistant professor of economics at Mississippi Southern College.

Joe S. Floyd, Jr., of the University of Florida, was elected a member of the board of editors of the Southern Economic Association at its last annual meeting.

John R. Franzman, of the University of Connecticut, has accepted an appointment as instructor in agricultural economics at Oklahoma A. and M. College.

John Gurley, associate professor of economics at the University of Maryland, is on leave to the Brookings Institution to develop a history and analysis of commercial banking in the United States.

Charles R. Halstead was appointed assistant professor of history and economics at Converse College last September.

Donald J. Hart, formerly at the University of Idaho, has been elected dean of the College of Business Administration of the University of Florida. He took office in February.

Alton L. Hawk has been appointed assistant professor of business law at the Georgia State College of Business Administration.

H. O. Jackson, formerly the owner of the Laurel Business College, is now assistant professor of business education at Mississippi Southern College.

E. A. J. Johnson, formerly deputy director of the United States Operations Mission to Yugoslavia, is serving as visiting professor of economics at the University of Maryland.

Earvin L. Joyner, formerly of the Southern Regional office staff in Atlanta, has been appointed employment analyst of the Bureau of Labor Statistics' newly established District office in Dallas, Texas.

Milton E. Kelley was on leave of absence from the Georgia State College of Business Administration during the winter quarter, 1956.

George E. Kiser, formerly of the University of Texas, has been named chairman of the School of Business, West Texas State College.

Mary H. Lane has been granted a leave of absence for one year from the Georgia State College of Business Administration.

L. R. LaVallee has resigned as associate professor of economics at Mississippi Southern College to accept a position in the Department of Economics at Dickinson College.

T. J. Leary, instructor, formerly of Ohio State University, joined the economics faculty of the University of Maryland last September.

Richard H. Leftwich has been promoted to professor of economics at Oklahoma A. and M. College.



Donald J. May has resigned as assistant professor of economics at the School of Business Administration, Emory University, to accept a position with the U. S. Department of Labor, Bureau of Labor Statistics, in Atlanta, Georgia, as research economist and chief of the region's inquiries and correspondence unit.

Albert D. Maynard has been granted a leave of absence for one year from the Georgia State College of Business Administration.

Guy T. Peden has been appointed instructor in business administration at Mississippi Southern College.

Jerome Peschke, professor of finance at the University of Houston, has been granted a leave of absence for the spring semester of the 1955-56 academic year.

Parley M. Pratt, formerly of the Ohio State University, has been named assistant professor of marketing in the College of Business Administration, University of Texas.

Donald A. Rece, formerly administrator of the hospital at Franklin, Virginia, has been appointed planning director for the Hospital Administration Program at the School of Business Administration, Emory University.

J. Ed Roche has been appointed associate professor of economic statistics at the Georgia State College of Business Administration.

G. D. Shelby, instructor, formerly of the University of California, joined the economics faculty of the University of Maryland last September.

Ernst W. Swanson, professor of economics at Emory University, was elected a member of the board of editors of the Southern Economic Association at its last annual meeting.

Sam B. Tidwell, professor of accounting and head of Department of Accounting at Mississippi Southern College, has been granted a six months' leave of absence to join the Public Service Administration in instituting accounting systems in several public school systems.

Robert L. Tontz has resigned as assistant professor of agricultural economics at Oklahoma A. and M. College to accept a position with the Agricultural Research Service, USDA, Washington, D. C.

Rudolph W. Trenton has been promoted to professor of economics at Oklahoma A. and M. College.

Carl Wiegand, associate professor of economics and finance at the University of Mississippi, is visiting professor of economics at the University of Illinois during the 1955-56 academic year.

Harry Williams, assistant professor of economics at the University of Houston, is to be on leave during the spring semester and summer session to complete work on his doctorate at the University of Texas.

Ben M. Wofford has assumed the position of acting associate director of the Bureau of Business Research at Mississippi State College.

James Wykle, formerly head of the Business Department, Sullivan County Board of Education, Blountville, Tennessee, is now instructor in business at Mississippi State College for Women.

Ervin K. Zingler, professor of economics and finance at the University of Houston, was elected a member of the board of editors of the Southern Economic Association at its last annual meeting.

## NEW MEMBERS

The following names have been added to the membership of the Southern Economic Association:

- Karl E. Ashburn, McNeese State College, Lake Charles, La.  
John E. Barthel, Stetson University, DeLand, Fla.  
William H. Baughn, Louisiana State University, Baton Rouge, La.  
Thomas S. Berry, 2514 Staples Mill Road, Richmond 28, Va.  
Claude A. Bitner, Jr., University of Texas, Austin, Tex.  
C. P. Blair, University of Texas, Austin, Tex.  
Edward R. Bowden, College of William and Mary, Williamsburg, Va.  
Elbert V. Bowden, Duke University, Durham, N. C.  
Thomas E. Brent, Texas A. and M. College, College Station, Tex.  
Francis J. Bridges, University of Alabama, University, Ala.  
Wanda M. Broussard, A. and T. College of North Carolina, Greensboro, N. C.  
Richard Bryan, Louisiana Polytechnic Institute, Ruston, La.  
J. D. Butterworth, University of Florida, Gainesville, Fla.  
James A. Byrd, University of Texas, Austin, Tex.  
Clyde C. Carter, University of North Carolina, Chapel Hill, N. C.  
Cleveland A. Chandler, Bluefield State College, Bluefield, W. Va.  
James E. Chapman, University of Alabama, University, Ala.  
Henry C. Chen, University of Houston, Houston, Tex.  
Sherrill Cleland, University of Richmond, Richmond, Va.  
James Carter Cooper, Texas A. and M. College, College Station, Tex.  
George F. Currie, Jackson College, Jackson, Miss.  
Ben F. Curry, Memphis State College, Memphis, Tenn.  
Lynn E. Dallenbarger, Jr., University of Florida, Gainesville, Fla.  
John A. Davis, University of Alabama, University, Ala.  
Austin M. Drumm, Lexington, Va.  
H. C. Edgeworth, Mississippi Southern College, Hattiesburg, Miss.  
Vernon R. Esteves, First Research Corporation, Miami, Fla.  
Amos W. Ford, Louisiana Polytechnic Institute, Ruston, La.  
Robert A. Ford, University of Alabama, University, Ala.  
Sergio R. Gazitua, University of Texas, Austin, Tex.  
A. A. George, Tougaloo Southern Christian College, Tougaloo, Miss.  
Georgia State College of Business Administration, Atlanta, Ga.  
Franklee Gilbert, University of North Carolina, Chapel Hill, N. C.  
Joseph C. Golden, Tennessee Polytechnic Institute, Cookeville, Tenn.  
Ralph T. Green, Baylor University, Waco, Tex.  
William E. Green, University of Mississippi, University, Miss.  
Oliver F. Guinn, University of Texas, Austin, Tex.  
Fawzi Habib, Duke University, Durham, N. C.  
Daniel E. Hahn, University of Alabama, University, Ala.  
William R. Hammond, North East Louisiana State College, Monroe, La.  
Mark Hanna, University of North Carolina, Chapel Hill, N. C.  
P. T. Hendershot, Louisiana Polytechnic Institute, Ruston, La.  
Robert E. Hill, University of Alabama, University, Ala.  
Billy J. Hinton, Baylor University, Waco, Tex.

- John Elton Hodges, Rice Institute, Houston, Tex.  
James M. Howell, Texas A. and M. College, College Station, Tex.  
Harold L. Jackson, University of California, Berkeley, Calif.  
Barclay G. Jones, University of North Carolina, Chapel Hill, N. C.  
Weldon D. Kettler, University of Houston, Houston, Tex.  
Vernon R. Kiely, Stetson University, DeLand, Fla.  
Wylie Kilpatrick, University of Florida, Gainesville, Fla.  
Richard A. LaBarge, Duke University, Durham, N. C.  
Marvin E. Lee, University of North Carolina, Chapel Hill, N. C.  
Charles W. Lewis, Alabama Polytechnic Institute, Auburn, Ala.  
G. E. Manners, Georgia State College of Business Administration, Atlanta, Ga.  
Thomas A. Martinsek, North Carolina State College, Raleigh, N. C.  
W. E. Mereness, The Macmillan Company, New York 7, N. Y.  
Jerome Wilson Milliman, Florida State University, Tallahassee, Fla.  
Oren G. Moore, Jr., University of Florida, Gainesville, Fla.  
Robert M. Moore, Texas A. and M. College, College Station, Tex.  
S. Stanley Morris, Grambling College, Grambling, La.  
Charles F. Nagy, 1186 Perkins Terrace, Memphis, Tenn.  
Warren B. Natim, University of Alabama, University, Ala.  
W. N. Peach, University of Oklahoma, Norman, Okla.  
Charles W. Pruitt, Jr., University of Florida, Gainesville, Fla.  
W. Clyde Robinson, Baylor University, Waco, Tex.  
Gaines M. Rogers, Wake Forest, Wake Forest, N. C.  
Broadus E. Sawyer, A. and T. College of North Carolina, Greensboro, N. C.  
Eric Schenker, University of Florida, Gainesville, Fla.  
Oury L. Selig, University of Texas, Galveston, Tex.  
James Shane, 1606 Fairchild, Manhattan, Kans.  
Allen N. Sievers, University of Florida, Gainesville, Fla.  
Alfred G. Smith, Jr., University of South Carolina, Columbia, S. C.  
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- modernization of all facts, figures, statistics, and analyses in conformance to the latest developments in the economy

The point of view remains the same: that our economy has such varied problems that no single approach is best suited in studying these problems. Analysis of national income, therefore, does not crowd out more traditional and still important issues. The text maintains a high level of sophistication and professional competence without being too difficult. It provides a wealth of up-to-date institutional material and, as before, description, analysis, and theory are expertly blended in dealing with the structure and functioning of today's American economy. Emphasis is on those aspects of economics that are going to be of most importance to the future.

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